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Explaining Charter School Effectiveness

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ABSTRACT

Explaining Charter School Effectiveness*

Estimates using admissions lotteries suggest that urban charter schools boost student achievement, while charter schools in other settings do not. Using the largest available sample of lotteried applicants to charter schools, we explore student-level and school-level explanations for this difference in Massachusetts. In an econometric framework that isolates sources of charter effect heterogeneity, we show that urban charter schools boost achievement well beyond that of urban public school students, while non-urban charters reduce achievement from a higher baseline. Student demographics explain some of these gains since urban charters are most effective for non-whites and low-baseline achievers. At the same time, non-urban charter schools are uniformly ineffective. Our estimates also reveal important school-level heterogeneity within the urban charter sample. A non-lottery analysis suggests that urban charters with binding, well-documented admissions lotteries generate larger score gains than under-subscribed urban charter schools with poor lottery records. Finally, we link charter impacts to school characteristics such as peer composition, length of school day, and school philosophy. The relative effectiveness of urban lottery-sample charters is accounted for by these schools' embrace of the No Excuses approach to urban education.

JEL Classification: I21, I24, I28, J45

Keywords: human capital, charter schools, achievement

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I Introduction

A growing body of evidence suggests that urban charter schools have the potential to generate impressive achievement gains, especially for minority students living in high-poverty areas. In a series of studies using admissions lotteries to identify causal effects, we looked at the impact of charter attendance in Boston and at a KIPP school in Lynn, Massachusetts (Abdulkadiroğlu et al., 2009, 2011; Angrist et al., 2010a, 2010b). Boston and Lynn charter middle schools increase student achievement by about 0.2 standard deviations (σ) per year in English Language Arts (ELA) and about 0.4σ per year in math. Among high school students, attendance at a Boston charter school increases student achievement by about 0.2σ per year in ELA and 0.3σ per year in math. Lottery studies of charter schools in the Harlem Children's Zone (Dobbie and Fryer, 2011a) and a Washington DC charter boarding school (Curto and Fryer, 2011) document similarly large gains. Studies of Chicago and New York charter schools also report positive effects (Hoxby and Rockoff, 2004; Hoxby, Murarka and Kang, 2009; Dobbie and Fryer, 2011b).

While these results are encouraging, they come from schools operating in traditional (for charters) urban settings. Interest in charter schools is growing rapidly in school districts outside central cities (see, e.g., the discussion of New York area charters in Hu, 2011), but results for more diverse sets of charter schools are also more mixed. In a recent report evaluating roughly two dozen Massachusetts charter schools from around the state, we find little evidence of achievement gains at schools outside of high-poverty urban areas (Angrist et al., 2011). Some of the estimates for non-urban Massachusetts charters show significant negative effects. These results echo findings from a multi-state study of 36 charter middle schools using admissions lotteries (Gleason et al., 2010).[1] Here too, charter schools outside of urban areas seem to do little for achievement, though, as in our earlier work, urban schools with high-minority, high-poverty enrollment generate some gains.[2]

This paper documents the magnitude of treatment effect heterogeneity in a large sample of Massachusetts charter schools and develops a framework for interpreting this heterogeneity using both student- and school-level explanatory variables. We begin with a semiparametric investigation of potential outcomes that assigns a role to variation in no-treatment counterfactuals and

[1] Other studies documenting heterogeneity in the effects of charter schools include Hoxby (2004), Zimmer et al. (2009), and Imberman (2011). The Imberman study reports that urban charters born as charters have large effects on discipline and attendance, while converted schools do not.

[2] A focus on differences between urban and non-urban schools also appears in research on Catholic schools. Evans and Schwab (1995) and Neal (1997) show that Catholic school attendance leads to increases in high school graduation and college attendance for cohorts graduating in the early 1980s. Both studies find larger benefits for black students and for students in urban settings. Grogger et al. (2000) and Altonji et al. (2005) report similar results on Catholic schooling for more recent cohorts.

to charter applicants' demographic characteristics and baseline achievement. This investigation includes a Blinder-Oaxaca (1973) decomposition of the urban charter advantage. The resulting estimates show that students at urban charters in the lottery sample are typical of the urban student population, and that urban charter attendance boosts achievement well beyond ambient non-charter levels. Student demographics and baseline scores play a role in this – urban schools work best for minority students and students with low baseline scores – but non-urban charters appear to be ineffective for most subgroups.

We then investigate school-level characteristics that might explain differences in charter school effectiveness. Our school-level investigation of charter effect heterogeneity is built on a set of non-lottery estimates that rely on statistical controls to eliminate selection bias. The observational analysis suggests that the sample of urban schools for which a lottery-based analysis is feasible, that is, over-subscribed schools with good lottery records, boost scores more than other urban charter schools. Finally, we show that urban and lottery-sample charter effectiveness can be explained by adherence to a No Excuses approach to urban education that emphasizes instruction time, comportment, selective teacher hiring, and focuses on traditional math and reading skills. Conditional on No Excuses status, factors such as time in school and per-pupil expenditure are not predictive of charter effectiveness. Interestingly, peer effects also play no role in explaining charter effectiveness.

The results reported here contribute to a growing body of evidence documenting the effectiveness of No Excuses practices in various contexts. Dobbie and Fryer (2011b) show that an index measuring teacher feedback, data-driven instruction, tutoring, increased instruction time, and high expectations is a significant predictor of effectiveness in a sample of New York charter schools. These practices are typically understood to be central elements of the No Excuses model (Thernstrom and Thernstrom, 2004; Carter, 2000).[3] Similarly, Fryer (2011) reports on an experiment implementing No Excuses strategies in nine low-performing traditional public schools in Houston. This intervention appears to have produced substantial gains, suggesting that the No Excuses model may be effective beyond the charter context.

The following section details school participation, describes the data, and outlines our empirical strategy for the lottery analysis. Section III presents the findings that motivate our investigation of charter effect heterogeneity. Section IV outlines the econometric framework used to investigate this heterogeneity and reports the results of this investigation. Section V discusses our observational analysis of the connection between charter effectiveness and school practices. Finally, we present a case study of a single non-urban charter school that embraces

[3]We note, however, that Dobbie and Fryer use a narrower definition of No Excuses that is limited to school disciplinary practices. They argue that disciplinary practices do not predict achievement gains after accounting for the elements measured by their index.

some elements of the urban No Excuses approach. This school does not produce gains comparable to urban No Excuses schools; in fact, attendance in the elementary school years seems to reduce achievement. Together, the findings reported here suggest that No Excuses pedagogy drives the success we observe among urban schools, but efficacy of the No Excuses package may depend on completeness of the package and/or factors specific to the urban environment.

II Lottery Analysis: Data and Empirical Strategy

Lottery and Survey Data

We attempted to collect lottery data for the set of Massachusetts charter schools serving middle and high school grades and meeting a set of pre-specified eligibility criteria.[4] The school-selection process is detailed in Table 1. Schools eligible for our study accept students in the relevant entry grades (4th-7th grade for middle school and 9th grade for high school). Excluded are closed schools, schools that opened after the 2009-2010 school year, and alternative schools serving non-traditional populations (usually students at risk of dropping out). The resulting set of eligible schools includes 28 of the 34 charters with middle school entry grades and eight of 16 schools with high school entry grades.[5] Eligible schools that are not included in the lottery analysis were either under-subscribed or failed to keep sufficient lottery records. The final sample of over-subscribed schools with usable records includes 17 middle schools and six high schools. These schools are listed in Appendix Table A1. The lottery sample includes nine urban middle schools, eight non-urban middle schools, four urban high schools, and two non-urban high schools.[6]

In an effort to document differences in school practice, we surveyed the full set of eligible charter schools, regardless of the quality of their lottery records. Twenty-nine school administrators completed this survey.[7] As shown in Panel A of Table 2, this survey reveals important differences between urban and non-urban charter schools. Urban schools are younger than non-urban schools; in Spring 2010, the average urban school had been open for 8.7 years, while the average non-urban school had been open for 12.4 years. Urban charter schools also run a longer school day and year than do non-urban schools. The average urban charter year lasts 189

[4]We focus on middle and high schools because data for elementary school lotteries are much less widely available. Moreover, pre-lottery test scores – a key component of the observational analysis – are unavailable for elementary school applicants.

[5]Three eligible schools serve both middle and high school grades, so there are 33 eligible campuses. Schools are classified as both middle and high if they have entrance lotteries at both levels, or if lottery records at the middle school level were available early enough for participants to be observed in high school.

[6]Urban areas are those in which the local district superintendent participates in the Massachusetts Urban Superintendents Network. The distinction between urban and non-urban charter schools in Massachusetts is essentially identical to splits based on high poverty or high minority enrollment.

[7]This generates data for 31 schools since two surveyed campuses admit in both middle and high school.

days and has a school day of 467 minutes, compared to 183 days and 422 minutes at non-urban schools. The extra time appears to go to increased math and reading instruction; urban schools spend 36 extra minutes per day on math and 37 extra minutes per day on reading. In addition, urban charter schools are 35 percentage points more likely to have Saturday school.

Urban and non-urban schools also differ with respect to school philosophy and organization. Urban charters are more likely than non-urban charters to require parents to sign a contract (72 percent compared to 46 percent), to require students to sign a contract (61 percent compared to 55 percent), and to use uniforms (89 percent compared to 73 percent). Urban charter schools are also much more likely to use a formal reward system to shape student behavior; 67 percent of urban schools use such a system, while only 18 percent of non-urban schools do so. The survey results reveal a particularly sharp division between urban and non-urban charters with respect to the No Excuses approach to education. As discussed by Thernstrom and Thernstrom (2003) and Carter (2000), No Excuses principles include a strict disciplinary environment, an emphasis on student behavior and comportment, extended time in school, an intensive focus on traditional reading and math skills, and teacher quality. Two-thirds of urban charter administrators identify somewhat or fully with No Excuses, while no non-urban charter identifies with this approach.

Table 2 also compares the inputs and resources used by urban and non-urban charter schools. All urban charters qualify for Federal Title I funds.[8] Urban charters spend about as much as traditional public schools, while non-urban charter schools spend less ($13,668 compared to $11,091).[9] Urban schools are more likely to hire paid tutors to work with their students. Involuntary teacher separations are higher in urban charters, but the requirement to take calls after hours and the use of unpaid tutors is less prevalent in urban schools. On the other hand, urban charter schools are substantially more likely to use paid tutors.

Differences in teacher characteristics across settings are also of interest. Compared to non-urban charters and public schools, urban charters have substantially younger teachers. This can be seen in Panel B of Table 2, which reports staff proportions under age 32 and over age 49. Probably due to these age differences, urban charter teachers are less likely to be licensed than traditional public school teachers. Student/teacher ratios at charter schools are generally smaller than staff ratios at traditional public schools, while non-urban charter schools have the smallest classes.

[8] Schools are eligible for Title I status if 40 percent of their students are from low-income families. See http://www2.ed.gov/programs/titleiparta/index.html for details.

[9] Column (4) shows selected characteristics of traditional public schools in the 2010-2011 school year, gathered from http://profiles.doe.mass.edu. Survey measures are unavailable for traditional public schools.

Student Data

The student-level data used here come from administrative records covering all Massachusetts' public schools.[10] Our sample covers the 2001-2002 school year through the 2010-2011 school year. The administrative records include information on demographics and school(s) of attendance, as well as Massachusetts Comprehensive Assessment System (MCAS) scores. The MCAS is a set of high-stakes standardized tests given to students in grades 3-8 and grade 10. The primary outcomes analyzed in our study are math and English Language Arts (ELA) scores. The data appendix provides details on the availability of outcomes for each applicant cohort. Raw MCAS scores were standardized by subject, grade level, and year.

The lottery analysis sample matches applicant records to administrative data using applicants' name, year, and grade. Where available, information on date of birth, town of residence, race/ethnicity, and gender was used to break ties. Ninety-two percent of applicants were matched. Applicants were excluded from the lottery analysis if they were disqualified from the lottery they entered (this mostly affected applicants to the wrong grade level). We also dropped siblings of current students, late applicants, and some out-of-area applicants.[11] Students missing baseline demographic information in the state database were dropped as well.

Descriptive Statistics

We begin with a statistical picture of the Massachusetts student population in traditional public and charter schools, presented in Table 3 separately for urban and non-urban areas. Traditional schools are defined as those that are not charters, alternative programs for older students, exclusively special education, exam, or magnet schools. The table shows average demographic characteristics, participation rates in limited English proficiency (LEP) and special education (SPED) programs, and average baseline test scores. Baseline (pre-charter enrollment) scores are from 4th grade for middle school and 8th grade for high school.

Traditional urban students are unlike traditional students in the rest of the state. Specifically, urban students are more likely to be black or Hispanic, to participate in LEP or SPED programs, and to receive a subsidized lunch. Urban students also have much lower baseline test scores than other public school students, with scores 0.43σ and 0.47σ below the state average in math and ELA at the middle school level, and 0.42σ and 0.39σ below the average for high school. In contrast, non-urban students score 0.21σ and 0.23σ above the middle school average; the

[10] Records are from the Student Information Management System, or SIMS. See the data appendix for details.

[11] Charter schools typically give priority to sibling applicants, as well as to students in the local school district or region. Our applicant risk sets (discussed in the next section) distinguish between in-area and out-of-area applicants for schools that take substantial numbers of both. Out-of-area applicants were dropped at schools with fewer than five out-of-area applicants.

corresponding non-urban advantages in high school are 0.27σ and 0.28σ.

Eligible charter school students who live in urban and non-urban areas are more similar to their peers in regular public schools than to one another. On the other hand, we see important differences by charter status as well. Urban charter schools serve a higher proportion of black students than do urban public schools. Urban charter students are also less likely to participate in LEP or SPED programs, or to qualify for a subsidized lunch. Charter school students in both urban and non-urban areas have slightly higher baseline test scores than their public school counterparts. Applicants to charter schools for whom we have lottery data are similar to the population of enrolled charter students in both urban and non-urban areas.

Empirical Strategy

The lottery-based identification strategy captures causal effects for applicants to over-subscribed charters with high-quality lottery records. The second-stage equation for the lottery analysis is

$$y_{igt} = \alpha_{2t} + \beta_{2g} + \sum_j \delta_j d_{ij} + X_i'\theta + \tau s_{igt} + \epsilon_{igt}, \qquad (1)$$

where y_{igt} is a test score for student i in grade g in year t, α_{2t} and β_{2g} are year and grade effects, X_i is a vector of pre-lottery demographic characteristics (race, special education, limited English proficiency, subsidized lunch status, and a female-minority interaction), and ϵ_{igt} is an error term. The set of d_{ij} includes a separate dummy variable for every combination of charter school lotteries (indexed by j) seen in the lottery sample. In what follows, we refer to these combinations as "risk sets." The variable of interest, s_{igt}, measures years spent in charter schools between application and test dates.[12] The parameter τ captures the causal effect of charter school attendance.

OLS estimates of equation (1) fail to capture causal effects if the decision to apply to or attend a charter school is correlated with unmeasured ability, motivation, or family background. We therefore use a dummy variable, Z_i, indicating lottery offers as an instrumental variable for time spent in charter school. The first stage for this 2SLS procedure is

$$s_{igt} = \alpha_{1t} + \beta_{1g} + \sum_j \kappa_j d_{ij} + X_i'\mu + \pi Z_i + \eta_{igt}, \qquad (2)$$

where π is the effect of a lottery offer on charter attendance. As in the second stage equation, the first stage includes risk set controls and baseline demographic characteristics, as well as year and

[12]Students who transfer public schools are assigned to the school attended longest in a given year. Students with any charter attendance are coding as having been in charter for the year. Students attending multiple charters in a given year are coded as having been a student at the charter school attended longest. The variable s_{igt} counts years spent at any charter school, including those without lottery records.

grade effects. Estimates for high school use 10th grade MCAS scores only, with standard errors clustered by school/grade/year. Estimates for middle school use all non-repeat post-lottery test scores through 8th grade and add a second layer of clustering at the student level.

Randomly assigned lottery offers are likely to be independent of student ability, motivation, or family background (within risk sets). The appendix presents evidence in support of the lottery-based identification strategy. Specifically, Table A2 shows that conditional on risk set, winning the lottery is uncorrelated with student characteristics. Table A3 shows that MCAS outcomes scores are available for roughly 90 percent of middle school applicants and 75 percent of high school applicants. Score availability is two points higher for lottery winners than losers in the middle school sample, but this imbalance is unlikely to be important for the estimates discussed below.

Differences in effectiveness between urban and non-urban charter schools are a primary focus of our analysis. Area-specific 2SLS estimates were constructed using equations of the form

$$y_{igt} = \alpha_{2t} + \beta_{2g} + \sum_j \delta_j d_{ij} + X_i'\theta + \tau_u s_{igt}^u + \tau_n s_{igt}^n + \epsilon_{igt}, \quad (3)$$

where s_{igt}^u and s_{igt}^n are years in urban and non-urban charter schools. The first stage for urban attendance can be written

$$s_{igt}^u = \alpha_{1t} + \beta_{1g} + \sum_j \kappa_j d_{ij} + X_i'\mu + \pi_u Z_i^u + \pi_n Z_i^n + \eta_{igt}, \quad (4)$$

where Z_i^u and Z_i^n indicate offers from urban and non-urban charters, with a similar specification for non-urban attendance.

III Lottery Estimates

The first stage estimates reported in column (1) of Table 4 show that, among applicants to charter middle schools, students who won a charter school lottery spent about 1 year more in a charter before being tested than did students who were not offered a seat. Applicants who won high school entrance lotteries spent about half a year longer in a charter school between application and testing than applicants who lost. These first stage estimates are similar to those reported in Abdulkadiroğlu et al. (2011) for a smaller sample of charter schools in Boston.

Second stage estimates for the full sample of lottery schools appear in column (2) of Table 4. These imply that a year of attendance at a lottery sample charter middle school increases ELA scores by about 0.08σ and math scores by about 0.21σ. The high school 2SLS estimates reveal larger causal effects, with score gains on the order of 0.22σ per year for ELA and 0.29σ

per year for math.[13]

Estimates for the full state sample mask considerable heterogeneity by urban status, a pattern documented in columns (3) through (6) of Table 4. Although first stages at urban and non-urban middle schools are similar, the corresponding second stage estimates differ markedly. 2SLS estimates for urban middle schools, reported in column (4) of Table 4, suggest these schools generate gains of about 0.15σ in ELA and 0.32σ in math per year enrolled. In contrast, estimates for non-urban charter middle schools are negative. In particular, as can be seen in column (6), charter students at non-urban middle schools appear to lose ground relative to their public school peers at a rate of 0.14σ per year in ELA and 0.12σ per year in math. Not surprisingly, high school lottery results for urban schools are similar to the statewide results (since only two of the high schools in the state sample are non-urban), showing large gains in math and ELA. On the other hand, 2SLS estimates for non-urban charter high schools are small, negative, and not significantly different from zero.[14]

Variation in charter effects across demographic subgroups is documented in Table 5, separately for urban and non-urban schools. Urban charter schools boost scores for most subgroups, though not uniformly. Girls realize slightly larger gains in math, while boys see slightly larger ELA gains. Black and Hispanic students benefit considerably from urban charter attendance in middle school, but the estimated math gains for whites are smaller, with no increase in whites' ELA scores. Urban charter middle schools appear to produce especially large achievement gains for students eligible for a subsidized lunch and for those with low baseline scores. Attendance at urban charter high schools increases math scores in every group and raises reading scores for everyone except whites, though estimates for small groups are imprecise.

Non-urban charter attendance fails to raise scores for most of the subgroups examined in Table 5, and appears to reduce achievement for girls, whites, and students with low baseline scores in middle school. Estimates for non-urban black and Hispanic students are negative in middle school, though not significantly different from zero. Most of the estimates for non-urban charter high schools are also negative, though effects here are less precise than those for non-urban middle school (high school estimates for blacks and Hispanics are omitted due to the small size of these non-urban groups).[15]

[13] Results for writing scores, not reported here, are similar to those for ELA. See our working paper for details (Angrist et al. 2011).

[14] As shown in columns (3) and (5), the first stage for urban high schools is smaller than the first stage for non-urban high schools. This difference reflects the fact that a larger proportion of the non-urban high school sample comes from entrance lotteries in middle grades, generating more potential years of charter exposure by the time applicants were tested in high school. One of two non-urban high schools admits students in middle school, while one of four urban high schools admits in middle school.

[15] Clustering the non-urban high school standard errors by school-grade-year as in Table 6 produced standard

IV Differences in Students

We investigate student- and school-level explanations for the striking difference in achievement effects at urban and non-urban charter schools. The student-level analysis is cast in a semiparametric framework with heterogeneous potential outcomes, indexed against a Bernoulli treatment, $D_i \in \{0, 1\}$, to indicate charter attendance. The Bernoulli setup focuses on heterogeneity while abstracting from nonlinearities that seem second-order in this context (since the first stage effects of lottery offers are similar in the two settings for middle school, while the corresponding second stage estimates differ dramatically). Let Y_{1i} and Y_{0i} denote potential test scores for student i in and out of charter schools. The observed outcome for student i is

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})D_i.$$

In other words, we observe Y_{0i} for applicants who don't go to charter school and Y_{1i} for those who do.

Our empirical work uses data from many school- and cohort-specific admissions lotteries, but the analysis of heterogeneity is explained with reference to a single lottery. Offers in this lottery are indicated by Z_i, as before. Potential treatment assignments, denoted D_{1i} and D_{0i}, tell us whether student i attends a charter school if he wins or loses the lottery. Offers are randomly assigned and assumed to affect test scores only through charter attendance, so the potential outcome vector $(Y_{1i}, Y_{0i}, D_{1i}, D_{0i})$ is independent of Z_i. We also assume that winning an entrance lottery can only make charter attendance more likely, so that $D_{1i} \geq D_{0i}\ \forall i$, with strict inequality for some students.

Under these assumptions, instrumental variables estimation using Z_i as an instrument for D_i in the sample of lottery applicants produces a local average treatment effect (LATE; Imbens and Angrist, 1994). Here, LATE is the effect of charter attendance for students induced to enroll in a charter school by winning an admissions lottery (the compliers, who have $D_{1i} > D_{0i}$). When computed separately for urban and non-urban students, IV estimates identify

$$\begin{aligned}\tau_\ell &\equiv \frac{E_\ell[Y_i|Z_i = 1] - E_\ell[Y_i|Z_i = 0]}{E_\ell[D_i|Z_i = 1] - E_\ell[D_i|Z_i = 0]} \\ &= E_\ell[Y_{1i} - Y_{0i}|D_{1i} > D_{0i}], \quad l \in \{u, n\};\end{aligned}$$

where ℓ indexes location, E_ℓ denotes an expectation over students in location ℓ, and u and n indicate urban and non-urban locations, respectively. This is LATE in each setting.

errors that were much smaller than classical homoskedastic 2SLS standard errors, suggesting a problem of finite-sample bias due to clustering. To avoid this bias, conventional standard errors are reported for the non-urban high school subgroup estimates.

We pinpoint three sources of student-level heterogeneity that might contribute to the difference between τ_u and τ_n. The first is the urban/non-urban difference in treated and non-treated counterfactuals (that is, distinct differences in average Y_{1i} and Y_{0i}). This investigation tells us whether the urban charter advantage reflects high scores in the treated state, low non-treated outcomes, or both. The second is variation in Y_{0i} across charter and non-charter students *within* each area. This tells us whether charter applicants and/or charter lottery compliers are unusual in either setting. Finally, we decompose the difference in charter effectiveness across areas into a component due to differences in student populations and a component due to differences in effectiveness conditional on student characteristics.

Gaps in Treatment and No-Treatment Counterfactuals

The urban charter advantage can be split into two parts, the first capturing differences in potential outcomes in the treated state (differences in Y_{1i}) and the second capturing differences in potential outcomes in the non-treated state (differences in Y_{0i}). Specifically, we have

$$\begin{aligned}\tau_u - \tau_n = & \underbrace{E_u[Y_{1i}|D_{1i} > D_{0i}] - E_n[Y_{1i}|D_{1i} > D_{0i}]}_{\gamma_1} \\ & - \underbrace{\left(E_u[Y_{0i}|D_{1i} > D_{0i}] - E_n[Y_{0i}|D_{1i} > D_{0i}]\right)}_{\gamma_0}.\end{aligned} \tag{5}$$

Here, γ_1 measures the difference in treated potential outcomes for compliers at urban and non-urban charter schools, while γ_0 measures the difference in non-treated potential outcomes between these two groups.

Pooling urban and non-urban charter applicants, we estimate γ_0 using

$$Y_i(1 - D_i) = \psi(1 - D_i) + \gamma_0(1 - D_i)U_i + \sum_j \delta_j d_{ij} + \epsilon_i \tag{6}$$

where U_i is an urban status indicator. The first stage equation for $1 - D_i$ is

$$1 - D_i = \sum_j \kappa_j d_{ij} + \sum_j \pi_j d_{ij} Z_i + \eta_i. \tag{7}$$

The first stage equation for the interaction between $1 - D_i$ and urban status uses the same specification as equation (7).[16] For a model without covariates, Abadie (2003) shows that 2SLS estimation of this type of system produces estimates of marginal mean counterfactuals for compliers; in this case, the 2SLS estimate is the mean of Y_{0i} for compliers. (We estimate γ_1 using a model that replaces $(1 - D_i)$ with D_i in equations (6) and (7).) Our parameterization differs

[16] Since applicants to urban and non-urban charter schools are disjoint sets, the main effect for urban status is collinear with the d_{ij} and therefore omitted.

from Abadie's in two ways. First, we're interested in the *difference* in marginal mean outcomes between urban and non-urban compliers: ψ equals the average of Y_{0i} for lottery compliers in non-urban areas, while $\psi + \gamma_0$ is the average of Y_{0i} for compliers in urban areas. Second, our estimating equation includes a saturated model for risk sets. In this case, the 2SLS estimands are weighted averages of mean Y_{0i} for compliers across risk sets, with weights proportional to the variance of the first-stage fitted values in the risk set (this is a consequence of Theorem 3 in Angrist and Imbens 1995).[17]

Columns (1) and (2) of Table 6 report 2SLS estimates of urban and non-urban charter effects using scores one year after application for middle school and 10th grade scores for high school. Column (3), which reports $\tau_u - \tau_n$, shows that the differences in middle school charter effects by urban status are 0.38σ in ELA and 0.66σ in math. Columns (4) and (5) show large differences in non-charter fallback between urban and non-urban compliers. The estimates of γ_0 imply that, when enrolled in public schools, non-urban middle school compliers outscore urban compliers by 0.67σ in ELA and 0.57σ in math. On the other hand, in charter schools, non-urban compliers outscore urban compliers by only 0.29σ in ELA, and urban compliers score 0.09σ higher in math (though this estimate is not statistically significant).

Panel B of Table 6 reports the corresponding estimates for high school. Just as in middle school, lower non-treated outcomes for urban compliers are part of the urban impact advantage, though differences in treated outcomes are larger in high school than in middle school. In public school, non-urban compliers outscore urban compliers by 0.87σ in ELA and 0.76σ in math. In charter schools, these gaps shrink to 0.50σ and 0.23σ (the latter estimate is not statistically significant). In other words, charter high school compliers start well behind non-urban compliers. Charter attendance closes most of this gap in middle school, especially for math, and some though not all of the corresponding gap in high school.

Non-treated Gaps in Urban and Non-urban Areas

Table 6 compares the no-treatment outcomes of urban compliers to the corresponding outcomes of non-urban compliers. By contrast, here we benchmark achievement in each area using the *local* non-charter mean. This tells us whether the urban charter advantage is driven by unusually low no-treatment outcomes for compliers, or whether urban lottery compliers are, in fact, typical of their milieu. Figure 1 illustrates the alternative scenarios we have in mind: the left panel describes a situation in which the achievement of untreated urban students is comparable to

[17] For example, the probability limit of the 2SLS estimate of ψ in equation (6) is

$$\psi = \sum_{j \in \mathcal{NU}} \left(\frac{f_j \cdot \pi_j^2 \cdot Var(Z_i | d_{ij}=1)}{\sum_k f_k \cdot \pi_k^2 \cdot Var(Z_i | d_{ik}=1)} \right) E[Y_{0i} | D_{1i} > D_{0i}, d_{ij} = 1]$$

where $\mathcal{NU}$ is the set of non-urban lotteries and f_j is the fraction of non-urban students in risk set j.

ambient non-charter achievement, while the right panel describes a situation in which the urban fallback is unusually low.

The econometric analysis of within-area counterfactuals begins with a decomposition of the urban and non-urban LATE as follows:

$$\tau_\ell = \underbrace{E_\ell[Y_{1i}|D_{1i} > D_{0i}] - E_\ell[Y_{0i}|D_i = 0]}_{\lambda_1^\ell} \qquad (8)$$
$$- \underbrace{(E_\ell[Y_{0i}|D_{1i} > D_{0i}] - E_\ell[Y_{0i}|D_i = 0])}_{\lambda_0^\ell}, \quad l \in \{u, n\}.$$

The term λ_0^ℓ is the difference in average Y_{0i} between lottery compliers and the general population of non-charter students in the relevant area. The term λ_1^ℓ is the difference between the treated outcomes of compliers and ambient non-charter achievement. For example, large λ_1^u and small λ_0^u mean that urban charters push their students beyond typical non-charter achievement in cities.

The decomposition in (8) is constructed using equations of the form

$$Y_i(1 - D_i) = \Delta(1 - D_i) + \sum_j \delta_j d_{ij} + \epsilon_i, \qquad (9)$$

estimated separately for urban and non-urban students, with the same first stage specification as equation (7). Here, the 2SLS estimand is a weighted average of Y_{0i} for lottery compliers across risk sets. To estimate $E_\ell[Y_{0i}|D_i = 0]$, we omit risk set controls and estimate equation (9) by OLS in a sample of students that includes both applicants and non-applicants. The OLS estimand is thus a simple average of Y_i for non-charter students in location ℓ. Assuming that mean Y_{0i} is constant across risk sets for compliers, λ_0^ℓ is the difference between the 2SLS and OLS estimates of Δ. λ_1^ℓ is estimated by replacing $(1 - D_i)$ with D_i in equation (9).[18]

Estimates of equation (9) appear in Table 7. Columns (1) through (4) show results for urban schools. Column (2) reports the average Y_{0i} for non-charter students, while column (3) shows λ_0^u, the difference in average outcomes for compliers and non-charter students. Estimates of λ_1^u, the difference between the treated outcomes of urban compliers and the ambient level of urban achievement, appear in column (4).[19] The estimates of λ_0^u suggest that urban lottery compliers

[18] Standard errors for the difference between the 2SLS and OLS estimates were constructed using a stacked data set that includes two copies of each observation. Let $h \in \{1, 2\}$ index halves of the data, and define $E_{ihq} = 1\{h = q\}$ for $q \in \{1, 2\}$. We estimate

$$Y_{ih}(1 - D_{ih}) = \Delta_{2SLS} \cdot (1 - D_{ih}) \cdot E_{ih1} + \Delta_{OLS} \cdot (1 - D_{ih}) \cdot E_{ih2} + \delta \cdot E_{ih2} + \sum_j \delta_j \cdot d_{ij} \cdot E_{ih1} + \epsilon_{ih},$$

instrumenting $((1 - D_{ih}) \cdot E_{ih1})$ with $(Z_{ih} \cdot E_{ih1})$, and clustering standard errors by i as well as school-grade-year.

[19] Middle school scores are from the year after the lottery for applicants and 6th grade for non-applicants; high school scores are from 10th grade, as always.

are positively selected from the urban middle school population, but the estimated gaps are small, and marginally significant only for middle school ELA (λ_0^u for high school is virtually zero in both subjects). Because urban charter compliers have non-charter achievement levels that are fairly typical of students in urban areas, the large score gains generated by urban charter schools can be attributed to high scores in the treated state.

Columns (7) and (8) of Table 7 report estimates of λ_0^n and λ_1^n for students at non-urban charter schools. As in urban areas, the non-charter achievement level of non-urban middle school compliers is slightly higher than that of students in the surrounding public schools. The ELA scores of non-urban middle school compliers in public schools exceed the ambient non-urban achievement level by a statistically significant 0.10σ, while the estimate of λ_1^n for ELA is a precisely estimated -0.09σ. This implies that non-urban charter middle schools move their students from atypically high ELA achievement levels to levels below those of non-urban public school students. Non-charter math achievement of non-urban middle school compliers is statistically indistinguishable from the ambient non-charter level, while non-urban charter attendance pulls compliers 0.14σ below the non-charter mean. The results for non-urban high school students show more positive selection (high λ_0^n) than in middle school. As can be seen by comparing columns (7) and (8) in Panel B, charter attendance leaves non-urban high school students essentially unchanged from this higher starting point.

Combined with the estimates of γ_0 and γ_1 in Table 6, these results paint a consistent picture of the urban charter advantage. Urban middle school charters push the scores of their students from a typically low level up to a level much closer to the achievement seen among non-urban charter students (the scenario sketched in the left panel of Figure 1). Non-urban charter middle schools reduce the scores of their students, in some cases markedly so. The corresponding results for high schools are like like those for middle schools in that urban charter high schools push their students beyond the level of achievement typical of urban public high schools. Non-urban charter high schools leave scores unchanged from a higher non-charter counterfactual baseline.

Accounting for Student Demographics

The role student demographics play in generating the urban charter advantage is explored with the help of a decomposition in the spirit of Blinder (1973) and Oaxaca (1973). The first step uses the methods of Abadie (2003) to identify a linear local average response function for lottery compliers conditional on a vector of observable demographic variables, X_i. Specifically, we have

$$E_\ell[Y_i|D_{1i} > D_{0i}, D_i, X_i, d_{ij}] = X_i'\theta_\ell + \omega_\ell D_i + D_i X_i'\rho_\ell + \sum_j \delta_j d_{ij}, \quad l \in \{u, n\}. \tag{10}$$

This equation has a causal interpretation because conditional on being a complier, treatment status (charter enrollment) is ignorable. Abadie (2003) shows that 2SLS using Bernoulli instruments for a Bernoulli treatment consistently estimates this sort of linear model for local average causal response.

Equation (10) generates the following parameterization of the urban/non-urban difference in charter school attendance effects:

$$\tau_u - \tau_n = (\omega_u - \omega_n) + \bar{X}_n'(\rho_u - \rho_n) + (\bar{X}_u' - \bar{X}_n')\rho_u, \tag{11}$$

where

$$\bar{X}_\ell \equiv E_\ell[X_i | D_{1i} > D_{0i}].$$

The last term in equation (11) captures the part of the urban charter advantage explained by differences in demographics. In particular, this term tells us how much urban charter effects are boosted by the urban demographic. The first two terms capture the component of the urban advantage attributable to differences in effects within demographic groups.

Here, as always, Blinder-Oaxaca decompositions can be presented in two ways. In this case, the urban/non-urban difference in charter school impact can be decomposed with differences in means weighted by non-urban charter effects instead of urban. Specifically, we can write

$$\tau_u - \tau_n = (\omega_u - \omega_n) + \bar{X}_u'(\rho_u - \rho_n) + (\bar{X}_u' - \bar{X}_n')\rho_n. \tag{12}$$

Like equation (11), this expression includes components associated with differences in demographics and differences in effectiveness conditional on demographics. The last term measures how much more effective non-urban charter schools would be if their students were demographically similar to the urban charter population.

We construct these decompositions by estimating

$$Y_i = X_i'\theta_\ell + \omega_\ell D_i + D_i X_i'\rho_\ell + \sum_j \delta_j d_{ij} + \epsilon_i$$

by 2SLS, separately for urban and non-urban applicants, with first stage

$$D_i = X_i'\mu_\ell + \pi_\ell Z_i + Z_i X_i'\zeta_\ell + \sum_j \kappa_j d_{ij} + \eta_i \tag{13}$$

for D_i and similar first stages for interaction terms involving D_i. The covariate vector, X_i, includes sex, race (white or non-white), special education status, free lunch status, and dummies for performance at the advanced, proficient, or needs improvement level on baseline math and ELA tests.[20] Complier means for each component of X_i are estimated using the kappa-weighting procedure described in Abadie (2003).

[20]These score categories are used to determine whether schools in Massachusetts meet the Adequate Yearly Progress (AYP) standard under No Child Left Behind (NCLB).

Blinder-Oaxaca decompositions suggest that favorable demographics enhance urban charter effectiveness, but differences in student populations do not fully account for the urban charter advantage. This can be seen in Table 8, which shows the components of equations (11) and (12) for middle schools. (The non-urban high school samples are too small to admit meaningful investigations of effect heterogeneity using this approach.) Column (1) shows the difference in charter middle school treatment effects by urban status.[21] Columns (2) and (3) report the components of decomposition (11), which multiplies the urban/non-urban difference in demographics by treatment effects for urban schools. Column (2) shows how urban effectiveness might change if urban schools were to serve the non-urban population. These results suggest that 47 percent of the urban advantage in ELA (0.18/0.39) can be explained by the level of student demographics. The corresponding estimate for math is 51 percent. Urban schools are especially effective for poor and minority students, and they serve more of these students than do non-urban schools. On the other hand, column (3) shows that even with the same student mix as non-urban charter schools, urban charters would be more effective than non-urban charters. The urban charter advantage can therefore be attributed to a combination of student demographics and larger treatment effects within demographic groups. As shown in columns (4) and (5), decomposition (12) produces qualitatively similar results, though the standard errors for this decomposition are much larger due to the relative imprecision of the estimated ρ_n.

V Differences in Schools

Our exploration of school-level heterogeneity in achievement effects builds on observational estimates, since this provides a larger sample of schools with more variation in characteristics and practices and allows us to compare effects for eligible charter schools with and without lottery records. The observational estimates use a combination of matching and regression to control for observed differences between students attending different types of schools. Specifically, students attending lottery-eligible charters are matched to a control sample with the same baseline school, baseline year, sex, and race. Charter students are matched if they fall into a cell that includes at least one regular public school student; likewise, regular public school students are matched if they fall into in a cell that includes at least one student in an eligible charter school. Every charter student in the matched sample is therefore compared to at least one demographically similar student from the same cohort and sending school. This procedure yields matches for 92 percent of students in eligible charter schools. To validate the observational research design, we compared lottery-based and observational results for schools where both are available. This

[21] These differences differ slightly from those reported in Table 6 because equation (13) imposes first stage coefficients that are constant across risk sets, while the earlier estimates allow the first stage coefficients to vary.

comparison was encouraging, as the two designs produced qualitatively similar estimates for most schools.[22]

An observational model with interaction terms captures variation in charter effects with student, school, and peer characteristics. Specifically, we estimate the following equation for student i from cell c, observed in grade g in year t:

$$y_{igtc} = \alpha_t + \beta_g + \iota_c + X_i'\theta + P_i'\gamma + M_i \cdot P_i'\delta + \tau_i s_{igt} + \epsilon_{igtc} \tag{14}$$

where

$$\tau_i = \phi_0 + \phi_1 M_i + P_i'\phi_2 + M_i \cdot P_i'\phi_3 + W_{s(i)}'\phi_4. \tag{15}$$

Here, s_{igt} measures years spent in eligible charter schools for student i from baseline through year t, while X_i is a vector of additional student characteristics including limited English proficiency, special education status, subsidized lunch status, and baseline test scores.[23] M_i is a dummy for minority status, while P_i is a vector of peer characteristics and $W_{s(i)}$ describes other school characteristics (for charter schools only).[24] Equation (14) is estimated in a pooled sample of middle and high school students, with standard errors double-clustered at the student and school levels.

Consistent with the findings reported in Table 4, estimates of equation (14) reveal substantially larger treatment effects at urban charter schools. As shown in columns (1) and (5) of Table 9, the urban advantage is roughly 0.11σ in ELA and 0.18σ in math. Interestingly, controlling for urban and other school characteristics, oversubscribed schools with high-quality lottery records also seem to be more effective than non-lottery schools: lottery-sample schools generate gains that are 0.10σ and 0.13σ larger than the effects of non-lottery schools. This is further evidence of the importance of school-level heterogeneity in charter effects.

Columns (2) and (6) in Table 9 show the results of adding instruction time (minutes per day and in the relevant subject) and per-pupil expenditures to the list of interactions. These variables are often thought to be part of the education production function. Motivated in part by the long days at successful charter schools (Pennington, 2007), the Massachusetts legislature recently authorized a pilot program to extend the school day by two hours in some

[22]The results of this validation exercise appear in Appendix Table A4. Observational estimates come from models including grade, year, and matching cell fixed effects, demographics and baseline scores, and variables measuring years spent in eligible lottery and non-lottery charter schools. Observational estimates for schools in the urban lottery sample are strikingly similar to the lottery results. The match between lottery and observational results for non-urban schools is not as good, with more positive observational estimates than generated by lottery methods. This seems unlikely to affect the main conclusions from the observational analysis, however.

[23]The observational regressions also control for years spent in ineligible charters and alternative schools.

[24]The main effect for M_i in equation (14) is absorbed by fixed effects for cells used in the match. Peer characteristics are jackknife means of baseline achievement and free lunch status for a student's classmates in the relevant grade and year, centered to have mean zero in the estimation sample.

traditional public schools. Per-pupil expenditure is also of longstanding interest to researchers and policy-makers; increasing per-pupil expenditure is sometimes seen as an alternative to structural reforms (Hanushek, 1997). As it turns out, however, these school-environment variables are unrelated to variation in charter school treatment effects and accounting for these measures of practice does little to account for the urban and lottery-sample advantages.

We next ask whether school approach or philosophy explains charter-effect heterogeneity. The estimates in columns (3) and (7) of Table 9 come from models that swap a No Excuses dummy for other measures of school practice.[25] No Excuses charter schools generate ELA and math gains that are 0.18σ and 0.27σ larger than the effects of other charters. Moreover, No Excuses status accounts fully for the urban and lottery sample advantages in both subjects: conditional on No Excuses status, with no other inputs included, the estimated urban and lottery coefficients are small and insignificant, while still reasonably precisely estimated.

Because charter students are (somewhat) positively selected, a possible explanation for charter effectiveness is a peer effect. Rothstein (2004) emphasizes the potential importance of peer effects for the achievement of low-income students:

> "Ambitions are contagious; if children sit next to others from higher social classes, their ambitions grow. This finding has been reconfirmed often. Lower-class children achieve less if the share of low-income children in their school is higher. The drop is most severe when the subsidized lunch population exceeds 40%. This truth has not changed since Brown vs. Board of Education."

Our findings for charter schools provide little support for this theory. As can be seen in columns (4) and (8) of Table 9, the signs of the peer coefficients suggest that score gains vary inversely with peer achievement and family income, though these interaction effects are not significant for non-minority students. The negative relationship between peer achievement and score gains is more pronounced for minorities, as evidenced by the significant negative interactions between minority status and peer baseline scores.[26] Importantly, No Excuses status remains a strong predictor of charter school effectiveness in models that include peer characteristics, while instructional time and spending play little role.[27]

[25]The No Excuses dummy is constructed from responses to this question: *Do you see your school as adhering to a particular approach or philosophy, such as No Excuses?*

[26]With peer characteristics included, the urban coefficient becomes negative, but this reflects linear extrapolation to a scenario where the peer variables are the same in urban and non-urban areas, a situation never observed in our data (the mean baseline score is markedly higher for every non-urban school).

[27]Appendix Table A5 replicates the observational findings related to No Excuses using the lottery sample. In the lottery analysis, peer characteristics are jackknife means for students in the same risk set (instead of the same school), and the instrument set includes interactions of lottery offer dummies and these variables.

Figure 2 summarizes the relationship between No Excuses practice and charter impacts. The observational estimates in this figure come from a version of equation (14) that estimates separate coefficients for time spent in each eligible charter school, omitting interactions with student, school, and peer characteristics. The lottery estimates come from a 2SLS regression that instruments time spent in each lottery-sample charter with school-specific offers. The figure plots school-specific math coefficients against the corresponding ELA coefficients, labeling points by school type and location. The No Excuses advantage is striking: With few exceptions, estimates for urban No Excuses charters are large and positive in both subjects, while the estimates for non-urban schools as well as urban schools not associated with No Excuses are small or negative.

Finally, we note that comportment and discipline are often said to be defining features of No Excuses charter schools, and of effective charters more generally (Lake et al. 2012). If urban charter effectiveness is due to the No Excuses approach, we might expect to see a marked impact on disciplinary outcomes in urban schools. Table 10, which reports 2SLS estimates of equation (1) for suspensions, truancy, and total days attended in the year following applicant lotteries, reveals large effects of urban charters on discipline and attendance. Urban charter attendance is estimated to increase suspensions by 0.7 days in middle school and more than a full day in high school. These treatment effects exceed mean suspension rates in the lottery sample (0.54 days for middle school and 0.47 days for high school). Estimates for both middle and high school show significant increases in out-of-school suspensions, and smaller (but still substantial) increases in in-school suspensions. Though imprecise, the results for truancy suggest that attendance at an urban charter high school may reduce days of unauthorized absence. Likely reflecting the longer school years at urban charter schools, urban charters increase days attended by 7.3 days in middle school and 15.9 days in high school.

In contrast with the results for urban charters, estimates for non-urban charter schools show little effect on discipline. Non-urban estimates for suspensions and truancy are small, and none are significantly different from zero. Non-urban charters do appear to increase total days attended, though the estimates here are smaller than those for urban charters. These results sharpen the distinction between urban and non-urban charters. Attendance at urban No Excuses charter schools produces large effects on discipline as well as achievement; attendance at other charter schools has little effect in either domain.

VI No Excuses Practices in a Non-Urban School

No Excuses explains the urban charter advantage in the sense that urban non-No Excuses charter schools are no more effective than non-urban charters. At the same time, the picture is incomplete since Massachusetts currently has no non-urban charter schools that fully embrace the No Excuses approach. We can't say, therefore, whether non-urban students would realize the same sort of gains that urban No Excuses students enjoy.

In an effort to paint a more complete picture of charter effect heterogeneity, we collected data from a K-12 non-urban charter school that incorporates many practices more commonly found in urban charters. This school, which we refer to as non-urban charter high school (NCHS), falls outside of our main sampling frame due to kindergarten entry, but it provides a unique opportunity to study elements of No Excuses practice in a non-urban setting. As shown in column (1) of Table 11, NCHS features an extended day, Saturday school, long periods of math and reading instruction, parent and student contracts, uniforms, and a reward system. On the other hand, the teacher age distribution and fraction licensed at NCHS look more non-urban than urban. As can be seen in column (2), NCHS has more minority students than most non-charters, though most NCHS students are white.

Our analysis of NCHS uses data from kindergarten entrance lotteries from 2004-7, as well as expansion lotteries held in two years in which the school added middle and high school seats. The kindergarten cohort analysis uses MCAS scores in grades 3 through 6, while the analysis of expansion lotteries uses scores in grades 5 through 8 and 10 (see the data appendix for details).[28] The effect of NCHS is estimated using a version of equation (1) that takes time spent at NCHS as the endogenous variable of interest, instrumented with a lottery offer dummy. The risk sets for this analysis correspond to interactions of application cohort and application grade.

Our analysis of NCHS generates mixed findings. The first stage estimates in column (3) show that that kindergarten lottery winners spent an average of 2.4 more years at NCHS than lottery losers prior to taking the MCAS. The corresponding 2SLS estimates imply that NCHS attendance reduces ELA and math scores by 0.05σ and 0.09σ, both significant effects. On the other hand, the 2SLS estimates for the middle and high school expansion lotteries are large and positive, though imprecise and not significantly different from zero. Although somewhat inconclusive, these results fail to show the sharp evidence of achievement gains generated by the most effective urban charters (including some for which a single-school analysis produces clear evidence of gains).

The relatively weak NCHS results suggest that the No Excuses treatment interacts favorably with the urban demographic mix. Few NCHS students are as disadvantaged as those attending urban charters. The Blinder-Oaxaca decomposition reported in Table 9 shows that urban charters are especially effective for a nonwhite high-poverty population with low baseline achievement. It's worth noting, however, that an analysis of NCHS impacts in demographic subgroups fails to uncover substantial positive effects, though the subgroup samples are small and the resulting estimates imprecise.

[28]Table A10 reports on lottery balance at NCHS; Table A11 reports on attrition.

A second possibility is that NCHS is not "No Excuses enough". An analysis of effects on suspension along the lines of that reported for the full sample in Table 10 shows low rates overall and no increase for those who attend NCHS (though NCHS' longer school year is reflected in a marginally significant impact on days attended). NCHS also employs older teachers than do the charter schools in our urban sample. Moreover, NCHS reported no involuntary teacher separations. Many of the most effective urban charter schools rely on an inexperienced though perhaps relatively flexible teaching staff, and are quick to replace teachers who appear to be struggling or otherwise ill-suited to the school's approach. Consistent with the notion that teacher matching is an important part of charter school effectiveness, Dobbie and Fryer (2011b) report that selective teacher hiring predicts charter school success, including at charter schools that identify with a No Excuses disciplinary approach. Likewise, Fryer's (2011) effort to give Houston public schools a No Excuses makeover includes an effort to replace veteran teachers deemed ineffective.

VII Conclusions

Massachusetts' urban charter schools generate impressive achievement gains, while non-urban charters are largely ineffective and appear to reduce achievement for some. Candidate explanations for this constellation of findings include the fact that urban charter schools serve larger shares of minority students in districts where the surrounding achievement level is generally low, keep their students in school longer, spend more money per-pupil, and are much more likely to identify with the No Excuses instructional approach than are non-urban schools. Our analysis examines the contribution of these student- and school-level factors to the urban charter advantage.

Massachusetts' urban charter schools, including the over-subscribed schools at the heart of our lottery analysis, serve a typical urban population characterized by low test scores and high poverty rates. On average, urban charters push their students well beyond the achievement levels characteristic of urban public school districts, while non-urban charter schools leave their students' achievement unchanged or diminished from a higher starting point. Urban charter schools are most effective for minorities, poor students, and low baseline achievers, so part of the urban charter advantage can be explained by student demographics. On the other hand, non-urban charter schools fail to show clear gains for any group; the urban advantage would likely remain were non-urban students more like those found in cities. Our analysis also reveals important heterogeneity within the set of urban schools. Over-subscribed schools with well-documented admissions processes are more effective than other urban charters.

Our analysis of the relationship between school characteristics and treatment effects suggests that adherence to the No Excuses paradigm can account for both the urban and lottery-sample charter advantages. Instruction time and per-pupil expenditures are not strongly correlated with school-specific impacts and do not explain differences in effectiveness after controlling for No Excuses status. Consistent with a No Excuses explanation of the urban charter advantage, the large achievement gains generated by urban charter schools are mirrored by substantial effects on disciplinary outcomes in the urban sample. Our case study of a non-urban K-12 school that uses many elements of the No Excuses model fails to reveal comparable achievement gains. This finding points to possibly important interactions between No Excuses and population characteristics, as well as the importance of behavioral standards and teacher characteristics, since disciplinary and hiring practices at this school appear to differ from those typical of urban charters.

Our negative estimates for non-urban charter middle schools raise the question of why, despite their unimpressive achievement effects, many of these schools are over-subscribed. One possibility is that parents misjudge the consequences of non-urban charter attendance. In a study of school choice, Rothstein (2006) argues that parental choice is driven primarily by levels of peer achievement rather than school effectiveness. Of course, non-urban charter schools may generate gains on dimensions that non-urban families value more than the skills measured by the MCAS, especially in view of the fact that most non-urban students do reasonably well in any case. Still, it seems unlikely that most non-urban parents would welcome a deterioration in basic skills. In ongoing work, we're studying other outcomes in an effort to determine whether the heterogeneous findings for achievement reported here have longer-term consequences. Preliminary results shows positive effects of urban charter high schools on SAT scores, suggesting the gains reported here persist in an important way.

Finally, it's worth noting that the charter school effect heterogeneity documented here is relevant to the ongoing debate over charter expansion. Like many states, Massachusetts caps the number of charter schools. The U.S. Department of Education is pressing states to lift these caps. In 2010, the Massachusetts state legislature passed a bill relaxing the charter cap for districts in the lowest decile of MCAS performance. This law gives priority to "proven providers" who have previously operated schools deemed to be successful, but does not clearly define success.[29] Our methods show how a distinction between effective and ineffective charters can be grounded in rigorous empirical analysis, while our results suggest that charter expansion policies favoring operators and pedagogical models with documented effectiveness increases the likelihood that charters will reduce achievement gaps.

[29]See Candal (2010) for a detailed discussion of the new Massachusetts charter school law.

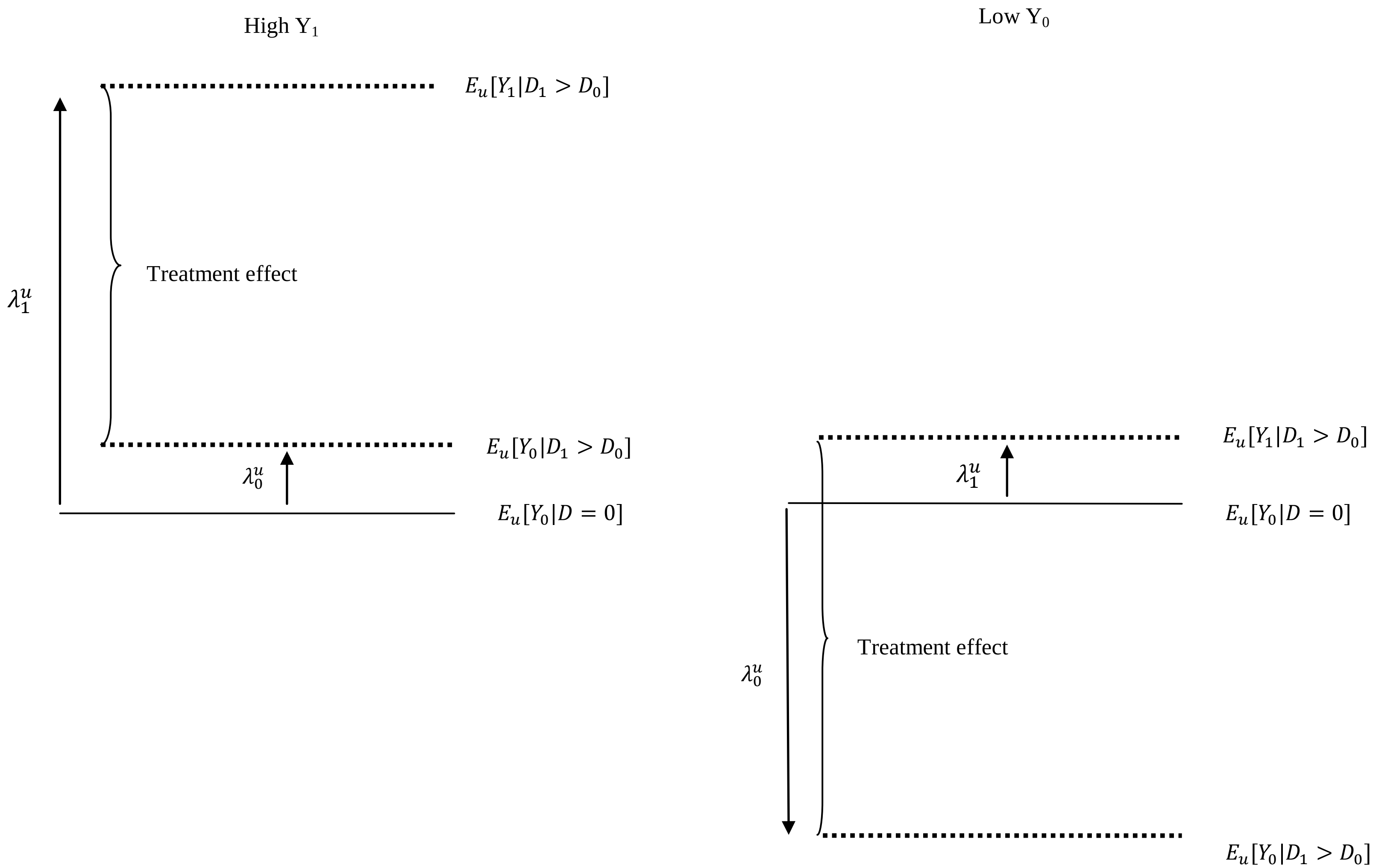
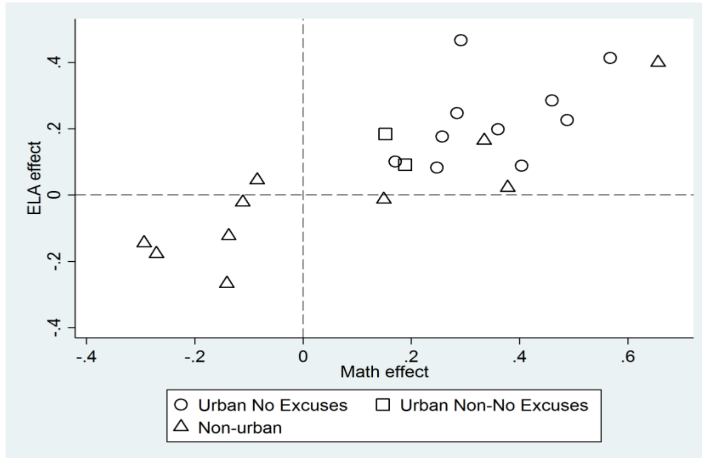


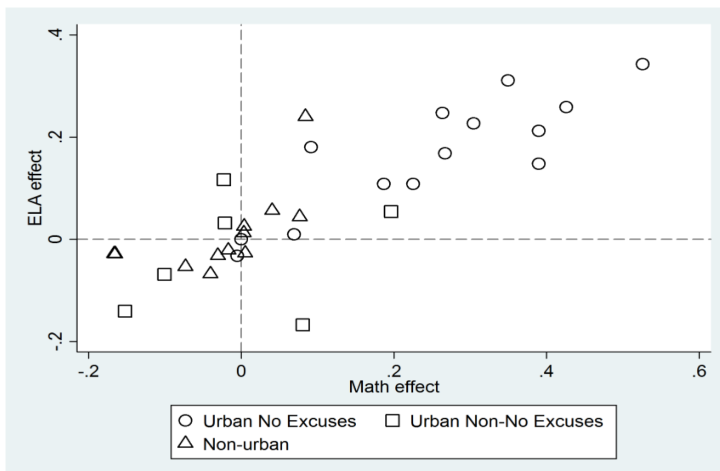
Figure 1: Treatment Effects in Urban Areas

Figure 2: School-specific Treatment Effects

A. Lottery Estimates



B. Observational Estimates



Notes: These figures plot school-specific math effects against school-specific ELA effects. Middle and high-school estimates are pooled to create the figures.

Table 1: School Participation

School level	Urban status	Boston status	Middle (entry in 4-7) and high (entry in 9) school charters* (1)	Charters eligible for lottery study (2)	Charters included in lottery study (3)
Middle	Urban		22	17	9
		Boston	12	9	7
		Non-Boston	10	8	2
	Nonurban		12	11	8
	Total (Urban and Nonurban)		34	28	17
High	Urban		12	6	4
		Boston	8	5	4
		Non-Boston	4	1	0
	Nonurban		4	2	2
	Total (Urban and Nonurban)		16	8	6

Notes: This table reports the number of middle and high charter schools in Massachusetts and their participation in the observational and lottery studies. The numbered notes below describe the schools included in each column. "Urban" towns are defined by the Massachusetts Department of Elementary and Secondary Education as the towns where the district superintendents participate in the Massachusetts Urban Superintendents Network. These towns include: Boston, Brockton, Cambridge, Chelsea, Chicopee, Everett, Fall River, Fitchburg, Framingham, Haverhill, Holyoke, Lawrence, Leominster, Lowell, Lynn, Malden, New Bedford, Pittsfield, Quincy, Revere, Somerville, Springfield, Taunton, and Worcester.

1. Middle and high charter schools in Massachusetts with the designated entry grades (in 4-7 and 9)*, including schools opened in 2010 and
2. Middle and high charter schools in Massachusetts with the designated entry grades (in 4-7 and 9)*, excluding closed schools, alternative
3. Middle and high charter schools that are included in column (3), excluding schools that are undersubscribed or have insufficient lottery

* There is an exception to the 9th grade entry criteria for high school. Two schools with lotteries at the middle school entry point which also enroll students in the high school grades are included in the high school sample.

Table 2: Characteristics of Charter and Public Schools

	All charters	Urban charters	Non-urban charters	Regular public schools
	(1)	(2)	(3)	(4)
Panel A. School Characteristics				
Years open	10.1	8.6	12.4	-
Days per year	186	189	183	-
Average minutes per day	450	467	422	-
Have Saturday school	0.310	0.444	0.091	-
Avg. math instruction (min)	82	96	60	-
Avg. reading instruction (min)	83	97	60	-
CMO or Network Affiliation	0.345	0.278	0.455	-
Fully or somewhat "No excuses"	0.414	0.667	0.000	-
Parent contract	0.621	0.722	0.455	-
Student contract	0.586	0.611	0.545	-
Uniforms	0.828	0.889	0.727	-
Reward system	0.483	0.667	0.182	-
Avg. per-pupil expenditure	$12,618	$13,668	$11,091	$13,047
Title I eligible	0.862	1.000	0.636	0.503
Fraction of teachers leaving voluntarily	0.089	0.081	0.101	-
Fraction of teachers leaving involuntarily	0.053	0.063	0.039	-
Unpaid tutors/volunteers	0.793	0.722	0.909	-
Paid tutors	0.103	0.167	0.000	-
Panel B. Teacher Characteristics				
Proportion of teachers 32 and younger	0.557	0.696	0.329	0.201
Proportion of teachers 49 and older	0.165	0.077	0.309	0.422
Proportion of teachers licensed to teach assignment	0.637	0.619	0.668	0.981
Proportion of core classes taught by highly qualified teachers	0.943	0.924	0.975	0.976
Student/teacher ratio	12.0	12.6	11.1	15.2
N (schools)	29	18	11	1810

Notes: This table reports characteristics of Massachusetts charter and traditional schools. Panel A shows the results of a survey of charter school administrators, while panel B shows teacher characteristics gathered from http://profiles.doe.mass.edu. Statistics are unweighted school-level means from the 2010-2011 school year. Column (1) reports results from the statewide sample of charter schools with entry in middle (4th-7th) or high school (9th) grades. The charter sample also excludes schools closed prior to Spring 2011, schools that opened after Spring 2010, and schools serving non-traditional student populations. Columns (2) and (3) show results for the uban and non-urban charter subsamples. Column (4) reports teacher characteristics for all traditional public schools in Massachusetts. Highly qualified teachers are teachers that possess a Massachusetts teaching license and demonstrate subject matter competency, either by passing a subject test or meeting one of several other criteria.

Table 3: Descriptive Statistics for Students

	Regular Public Schools		Charter schools (eligible)		Charter applicants (lottery)	
	Urban	Non-urban	Urban	Non-urban	Urban	Non-urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Middle Schools (5th-8th grade)						
Female	0.486	0.488	0.501	0.478	0.496	0.509
Black	0.183	0.027	0.381	0.035	0.479	0.022
Hispanic	0.319	0.038	0.246	0.039	0.233	0.025
Special education	0.191	0.165	0.167	0.158	0.176	0.185
Subsidized lunch	0.687	0.146	0.642	0.211	0.686	0.103
Limited English proficiency	0.160	0.017	0.082	0.022	0.085	0.008
Baseline Math score	-0.427	0.210	-0.322	0.259	-0.356	0.305
Baseline ELA score	-0.466	0.232	-0.312	0.275	-0.375	0.391
Years in charter	0.00	0.00	2.09	1.97	1.59	1.25
N (students)	171703	415794	8388	9070	4155	1701
N (schools)	262	400	17	11	9	8
Panel B. High Schools (10th grade)						
Female	0.499	0.494	0.557	0.545	0.548	0.538
Black	0.189	0.028	0.527	0.021	0.614	0.028
Hispanic	0.275	0.034	0.183	0.010	0.257	0.017
Special education	0.172	0.156	0.166	0.109	0.178	0.114
Subsidized lunch	0.612	0.126	0.608	0.146	0.717	0.123
Limited English proficiency	0.094	0.009	0.024	0.004	0.035	0.003
Baseline Math score	-0.420	0.268	-0.371	0.321	-0.320	0.440
Baseline ELA score	-0.392	0.278	-0.318	0.412	-0.315	0.552
Years in charter	0.00	0.00	1.77	1.81	0.64	1.30
N (students)	132774	357733	2676	909	3029	351
N (schools)	104	316	6	2	4	2

Notes: This table reports descriptive statistics for the sample of public school students (columns 1 and 2), the sample of students in eligible charter schools (columns 3 and 4), and the sample of charter applicants (columns 5 and 6) from 2002-2011. The sample is restricted to students in Massachusetts public schools at baseline with at least one followup test score. The number of schools in columns (1) and (2) is counted in 6th grade for middle school and 10th grade for high school. Years in charter school is measured as time spent in eligible charter schools through 8th grade for middle school and 10th grade for high school.

Table 4: Lottery Results

Subject		All charter schools: First Stage (1)	All charter schools: 2SLS (2)	Urban charter schools: First Stage (3)	Urban charter schools: 2SLS (4)	Non-urban charter schools: First Stage (5)	Non-urban charter schools: 2SLS (6)
Panel A. Middle School							
ELA		1.02***	0.075***	1.04***	0.146***	1.00***	-0.144***
		(0.040)	(0.025)	(0.051)	(0.028)	(0.074)	(0.039)
	N	16285		11649		4636	
Math		1.02***	0.213***	1.03***	0.321***	1.01***	-0.123***
		(0.040)	(0.028)	(0.051)	(0.031)	(0.074)	(0.047)
	N	16543		11941		4602	
Panel B. High School							
ELA		0.542***	0.215***	0.479***	0.280***	1.14***	-0.048
		(0.083)	(0.063)	(0.088)	(0.071)	(0.187)	(0.059)
	N	4101		3565		536	
Math		0.542***	0.285***	0.479***	0.360***	1.13***	-0.023
		(0.083)	(0.075)	(0.088)	(0.083)	(0.188)	(0.071)
	N	4048		3517		531	

Notes: This table reports 2SLS estimates of the effects of time spent in charter schools on test scores. The endogenous variable is years spent in charter schools, and the instrument is a lottery offer dummy. Columns (1)-(2) show estimates for all schools, columns (3)-(4) show estimates for urban charter schools, and columns (5)-(6) show estimates for non-urban schools. The urban and non-urban estimates for a given subject come from a single regression with two endogenous variables, using urban and non-urban offers as instruments. All models control for race, sex, special education, limited English proficiency, subsidized lunch status, and a female by minority dummy. Year of birth, year of test, and risk set dummies are also included. Middle school regressions pool post-lottery outcomes from 4th through 8th grade and cluster by student identifier as well as school-grade-year. High school regressions include only scores for 10th grade and cluster by school-grade-year.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 5: Lottery Results for Subgroups

			Sex		Race		Subsidized lunch	Lowest baseline quartile
			Female	Male	Black/Hispanic	White		
School level	Subject		(1)	(2)	(3)	(4)	(5)	(6)
					Panel A. Urban Schools			
Middle	ELA		0.124***	0.173***	0.211***	0.034	0.182***	0.279***
			(0.037)	(0.040)	(0.037)	(0.047)	(0.033)	(0.057)
		N	5852	5797	8176	2537	7992	2840
	Math		0.379***	0.276***	0.421***	0.133**	0.348***	0.388***
			(0.042)	(0.041)	(0.040)	(0.054)	(0.036)	(0.055)
		N	5994	5947	8415	2583	8182	2869
High	ELA		0.245***	0.303***	0.337***	-0.092	0.291***	0.274*
			(0.082)	(0.113)	(0.072)	(0.399)	(0.077)	(0.152)
		N	1954	1611	3059	311	2556	768
	Math		0.384***	0.345***	0.395***	0.244	0.359***	0.455***
			(0.103)	(0.128)	(0.082)	(0.411)	(0.089)	(0.119)
		N	1927	1590	3016	310	2520	810
					Panel B. Non-urban Schools			
Middle	ELA		-0.169***	-0.114*	-0.241	-0.150***	-0.119	-0.188**
			(0.049)	(0.060)	(0.251)	(0.040)	(0.099)	(0.076)
		N	2348	2288	237	4169	466	1134
	Math		-0.159***	-0.084	-0.230	-0.115**	-0.128	-0.159**
			(0.060)	(0.070)	(0.285)	(0.045)	(0.127)	(0.071)
		N	2332	2270	236	4135	456	1072
High	ELA		0.003	-0.088	-	-0.025	-0.204	0.109
			(0.099)	(0.092)		(0.070)	(0.222)	(0.130)
		N	281	255		496	71	123
	Math		-0.046	0.030	-	0.026	0.457*	-0.057
			(0.116)	(0.127)		(0.082)	(0.243)	(0.177)
		N	281	250		494	68	119

Notes: This table reports 2SLS estimates of the effects of time spent in charter schools for subgroups of students. All regressions include year dummies, grade dummies, risk set dummies, and demographic controls. Middle school standard errors are clustered on student identifier as well as school-grade-year. High school standard errors are clustered by school-grade-year for urban schools and are unclustered for non-urban schools.
*significant at 10%; **significant at 5%; ***significant at 1%

Table 6: Gaps in Treatment and No-treatment Counterfactuals

Subject		Urban effect (1)	Non-urban effect (2)	Effect difference (3)	Differences in potential outcomes: Y_0 (4)	Differences in potential outcomes: Y_1 (5)
Panel A. Middle School						
ELA		0.188***	-0.188***	0.376***	-0.666***	-0.291***
		(0.064)	(0.054)	(0.084)	(0.077)	(0.057)
	N	4551	2323			
Math		0.483***	-0.177**	0.659***	-0.570***	0.090
		(0.074)	(0.074)	(0.105)	(0.082)	(0.064)
	N	4858	2239			
Panel B. High School						
ELA		0.438***	0.064	0.373*	-0.873***	-0.500***
		(0.137)	(0.151)	(0.204)	(0.181)	(0.126)
	N	3790	435			
Math		0.590***	0.065	0.526**	-0.758***	-0.232
		(0.162)	(0.146)	(0.218)	(0.178)	(0.156)
	N	3743	432			

Notes: This table estimates components of the difference in charter treatment effects by urban status due to differences in non-charter "fallback" and differences in treated outcomes. Outcomes are test scores the year after the lottery. Columns (1) and (2) display urban and non-urban charter treatment effects, and column (3) gives the difference. Column (4) shows an estimate of the difference in average Y_0 between urban and non-urban compliers, computed as described in the text. Column (5) shows an estimate of the difference in Y_1 between these two groups.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 7: Potential Outcome Gaps in Urban and Non-urban Areas

		Urban				Non-urban			
		Treatment effect	$E_u[Y_0\|D=0]$	λ_0^u	λ_1^u	Treatment effect	$E_n[Y_0\|D=0]$	λ_0^n	λ_1^n
Subject		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Middle School									
ELA		0.188***	-0.422***	0.118**	0.306***	-0.188***	0.260***	0.102**	-0.086***
		(0.064)	(0.012)	(0.054)	(0.049)	(0.054)	(0.007)	(0.050)	(0.030)
	N	4551	117601			2323	316953		
Math		0.483***	-0.399***	0.077	0.560***	-0.177**	0.236***	0.010	-0.143***
		(0.074)	(0.011)	(0.049)	(0.054)	(0.074)	(0.007)	(0.061)	(0.042)
	N	4858	160394			2239	370813		
Panel B. High School									
ELA		0.417***	-0.369***	-0.004	0.410***	0.064	0.250***	0.237	0.301***
		(0.140)	(0.018)	(0.096)	(0.119)	(0.151)	(0.008)	(0.152)	(0.051)
	N	3790	143583			435	362206		
Math		0.557***	-0.371***	0.074	0.602***	0.065	0.241***	0.207	0.271***
		(0.164)	(0.021)	(0.099)	(0.151)	(0.146)	(0.008)	(0.145)	(0.041)
	N	3743	141468			432	360278		

Notes: This table compares potential outcomes for compliers and traditional public school students. For lottery applicants, outcomes are test scores in the year after the lottery for middle school and in 10th grade for high school. For non-applicants, outcomes are 6th grade scores in middle school, and 10th grade scores in high school. The treatment is a dummy for charter attendance. Columns (1) and (5) show 2SLS estimates of the effect of charter attendance on test scores in urban and non-urban areas, with the lottery offer dummy interacted with risk sets as instruments and risk sets as maintained controls. Columns (2) and (6) shows average test scores for non-charter students, including non-applicants. Columns (3) and (7) show differences between the average non-charter scores of compliers and non-charter students. Columns (4) and (8) show differences between the treated outcomes of compliers and the scores of non-charter students.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 8: Decomposing Differences in Impact for Middle School

		Decomposition 1 (urban loading)		Decomposition 2 (non-urban loading)	
	Urban vs. non-urban difference in TE	Due to diffs in cov. levels	Due to diffs in cov-specific TE	Due to diffs in cov. levels	Due to diffs in cov-specific TE
Subject	(1)	(2)	(3)	(4)	(5)
ELA	0.389***	0.184**	0.205**	0.197	0.192
	(0.073)	(0.077)	(0.095)	(0.146)	(0.180)
N	5734				
Math	0.633***	0.322***	0.312***	0.250	0.383*
	(0.079)	(0.075)	(0.094)	(0.163)	(0.200)
N	5731				

Notes: This table decomposes the difference between urban and non-urban charter treatment effects. Outcomes are test scores the year after the lottery. The treatment is a dummy for charter attendance. Column (1) shows the difference in urban vs. non-urban treatment effects, computed as described in the text. Columns (2) and (3) report the components of the urban/non-urban difference due to differences in covariate levels and differences in covariate-specific effects, weighting the difference in covariate means by the urban treatment effects. Columns (4) and (5) report a decomposition that weights the difference in means by the non-urban treatment effects. The covariates used in the decompositions are race (white vs. non-white), sex, special education, free/reduced price lunch, and baseline score categories (advanced, proficient, needs improvement, warning) in math and ELA.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 9: Observational Models for Charter School Effects

	ELA				Math			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	-0.078***	-0.158***	-0.029*	-0.010	-0.103**	-0.261**	-0.029	-0.050
	(0.029)	(0.051)	(0.015)	(0.046)	(0.043)	(0.104)	(0.025)	(0.060)
Urban	0.111***	0.113***	0.012	-0.055*	0.179***	0.135**	0.027	-0.102***
	(0.027)	(0.032)	(0.019)	(0.030)	(0.037)	(0.067)	(0.038)	(0.033)
Lottery	0.095***	0.055*	0.025	0.040*	0.129***	0.081**	0.023	0.063**
	(0.030)	(0.032)	(0.017)	(0.022)	(0.044)	(0.035)	(0.030)	(0.027)
High school	0.088***	0.082**	0.081***	0.087***	0.027	0.040	0.018	0.060
	(0.028)	(0.033)	(0.024)	(0.024)	(0.043)	(0.057)	(0.036)	(0.040)
Minutes per day/60	-	0.010	-	0.008	-	0.009	-	0.007
		(0.013)		(0.008)		(0.028)		(0.014)
Minutes in subject/60	-	-0.007	-	-0.005	-	0.093	-	0.107*
		(0.047)		(0.022)		(0.130)		(0.063)
PPE/1000	-	0.004	-	-0.005	-	0.003	-	-0.011**
		(0.005)		(0.003)		(0.008)		(0.005)
No Excuses	-	-	0.176***	0.130***	-	-	0.270***	0.196***
			(0.032)	(0.020)			(0.049)	(0.037)
Peer baseline score in subject	-	-	-	-0.009	-	-	-	-0.029
				(0.019)				(0.026)
Peer fraction free lunch	-	-	-	0.067*	-	-	-	0.064
				(0.040)				(0.042)
Black/hispanic	-	-	-	0.080***	-	-	-	0.100***
				(0.016)				(0.018)
Black/hispanic*peer baseline score	-	-	-	-0.060**	-	-	-	-0.082**
				(0.029)				(0.036)
Black/hispanic*peer fraction free lunch	-	-	-	-0.070	-	-	-	-0.038
				(0.058)				(0.066)
N (scores)	388316	381266	388316	381266	402333	395068	402333	395068
N (students)	140705	139554	140705	139554	140955	139936	140955	139936

Notes: This table shows OLS estimates of the effects of time spent in eligible charter schools and its interactions with school, student, and peer characteristics. The middle school and high school observational samples are stacked. All models control for grade effects, year effects, matching cell effects, student characteristics (SPED, LEP, FRPL, and baseline scores), and years in ineligible charters and alternative schools. The interaction terms for school and peer characteristics are means across all eligible schools attended. Peer characteristics are computed via jacknife (that is, each student is omitted from the calculation sthat produce her own peer means), and are mean zero in the estimation sample. Standard errors are double-clustered by student and school.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 10: Effects on Discipline and Attendance

		Urban		Non-urban	
		Mean	2SLS	Mean	2SLS
School level	Outcome	(1)	(2)	(3)	(4)
Middle	Total days suspended	0.537	0.710***	0.080	-0.016
			(0.080)		(0.040)
	N		5123		2641
	Days of in-school suspension	0.073	0.172***	0.020	-0.002
			(0.034)		(0.016)
	N		5123		2641
	Days of out-of-school suspension	0.464	0.538***	0.060	-0.014
			(0.067)		(0.032)
	N		5123		2641
	Days truant	0.594	0.128	0.235	-0.111
			(0.208)		(0.214)
	N		5123		2641
	Total days attended	173	7.28***	171	5.52***
			(1.19)		(1.59)
	N		5175		2605
High	Total days suspended	0.465	1.27***	0.126	-0.100
			(0.194)		(0.080)
	N		3582		533
	Days of in-school suspension	0.086	0.277***	0.036	-0.024
			(0.076)		(0.023)
	N		3582		533
	Days of out-of-school suspension	0.379	0.993***	0.090	-0.075
			(0.162)		(0.073)
	N		3582		533
	Days truant	5.69	-11.5	0.305	-0.370
			(7.58)		(1.36)
	N		3582		533
	Total days attended	163	15.9***	168	9.41*
			(4.22)		(5.47)
	N		3647		530

Notes: This table reports 2SLS estimates of the effects of charter school attendance on disciplinary outcomes and attendance in the year after the lottery. Standard errors are clustered by school-grade-year.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 11: Practices and Effects at NCHS

Practices and teacher characteristics		Student characteristics			Lottery Estimates		
						First stage	2SLS
Variable name	(1)	Variable name		(2)	Subject	(3)	(4)
Average minutes per day	455	Female		0.520		*Kindergarten Lotteries*	
Have Saturday school	Yes	Black		0.137	ELA	2.49***	-0.058**
Avg. math instruction (min)	90	Hispanic		0.025		(0.243)	(0.026)
Avg. reading instruction (min)	90	Special education		0.143			
Parent contract	Yes	Subsidized lunch		0.119	Math	2.47***	-0.097***
Student contract	Yes	Limited English proficiency		0.009		(0.244)	(0.032)
Uniforms	Yes		N	871	N	1243	1244
Reward system	Yes					*Expansion Lotteries*	
Fraction of teachers leaving voluntarily	0.131				ELA	0.519***	0.199
Fraction of teachers leaving involuntarily	0.000					(0.112)	(0.219)
Teachers licensed to teach assignment	0.803						
Proportion of teachers 32 and younger	0.440				Math	0.520***	0.254
Proportion of teachers 49 and older	0.240					(0.113)	(0.262)
					N	258	257

Notes: This table reports survey results, teacher characteristics, descriptive statistics, and lottery-based 2SLS estimates for NCHS. Column (1) shows selected survey responses and teacher characteristics. Column (2) shows descriptive statistics for all students attending NCHS in 4th grade between 2002 and 2011. Columns (3) and (4) show 2SLS estimates of the effects of NCHS attendance on test scores, instrumenting for years at NCHS with a lottery offer dummy. The 2SLS models control for year effects, grade effects, and risk set effects. Standard errors are clustered at the student level. The kindergarten lottery estimates use scores in grades 3-6, while the expansion lottery estimates use scores in grades 5-8 and 10.

Data Appendix

The data used for this study come from charter school lottery records, student demographic and school attendance information in the Massachusetts Student Information Management System (SIMS), and test scores from the Massachusetts Comprehensive Assessment System (MCAS) database. This appendix describes each data source and details the procedures used to clean and match them. The steps used here are an updated version of the methods described in the data appendix to Angrist et al. (2010).

A Data Sets

A.1 Charter School Entrance Lotteries

Data description and sample restrictions

Our sample of applicants is obtained from records of lotteries held at 20 Massachusetts charter schools between 2002 and 2010. The participating schools and lottery years are listed in Table A1, along with schools eligible for the lottery study that did not contribute records. A total of 100 school-specific entry cohorts are included in the analysis. Lotteries at three schools contribute observations to both the middle and high school samples.

The raw lottery records typically include applicants' names, dates of birth, contact information, and other information used to define lottery groups, such as sibling and out-of-area status. The first five rows in each panel of Table A6 show the sample restrictions we impose on the raw lottery records, separately by lottery cohort and school level. We exclude duplicate applicants and applicants listed as applying to the wrong entry grade. We also drop late applicants, out-of-area applicants, and sibling applicants, as these groups are typically not included in the standard lottery process. Imposing these restrictions reduces the number of middle school lottery records from 13,038 to 11,220 and reduces the number of high school records from 9,506 to 9,009.

Lottery offers

In addition to the data described above, the lottery records also include information regarding offered seats. We used this information to reconstruct indicator variables for whether lottery participants received randomized offers. For most schools and years, we code the offer variable as one for applicants who received offered seats at any time after the lottery, including offers to waitlisted students. This definition corresponds to the "ever offer" instrument used by Abdulkadiroglu et al. (2011). For a few schools, information on waitlist offers was unavailable, but records were sufficient to determine the students who received offers on the day of the lottery. The offer variables for these schools are coded as one for the initially offered students and zero otherwise. The instrument Z_i used in our analyses is one for any student who received an offer from any school included in our lottery sample. Offer rates were 67 percent and 64 percent in our middle and high school samples, respectively.

A.2 Student Information Management System Data

Data description

Our study uses SIMS data from the 2001-2002 school year through the 2010-2011 school year. Each year of data includes an October file and an end-of-year file. The SIMS records information on demographics and schools attended for all students in Massachusetts' public schools. An observation in the SIMS refers to a student in a school in a year, though there are some student-school-year duplicates for students that switch grades or programs within a school and year.

Coding of demographics and attendance

The SIMS variables used in our analysis include grade, year, name, town of residence, date of birth, sex, race, special education and limited English proficiency status, free or reduced price lunch, and school attended. We constructed a wide-format data set that captures demographic and attendance information for every student in each year in which he or she is present in Massachusetts' public schools. This file uses information from the longest-attended school in the first calendar year spent in each grade. Attendance ties were broken at random; this affects only 0.007 percent of records. Students classified as SPED, LEP, or free/reduced price lunch in any record within a school-year-grade retain that designation for the entire school-year-grade.

We measure charter school attendance in calendar years. A student is coded as attending a charter school in a particular year when there is any SIMS record reporting charter attendance in that year. Students who attend more than one charter school within a year are assigned to the charter they attended longest.

A.3 Massachusetts Comprehensive Assessment System Data

Data description and sample restrictions

We use MCAS data from the 2001-2002 school year through the 2010-2011 school year. Each observation in the MCAS database corresponds to a student's test results in a particular grade and year. We use math and English Language Arts (ELA) tests in grades 3 through 8 and 10, as well as Writing Topic and Writing Composition scores in grades 4, 7, and 10. The test score variables are standardized to have mean zero and standard deviation one within a subject-grade-year in Massachusetts. Repetitions of the same test subject and grade are dropped. In cases with multiple records within a year and grade, ties are broken at random; this affected 0.10 percent of MCAS records.

In the lottery-based middle school analysis, all post-lottery test scores through 8th grade are used as outcomes. High school outcomes are from 10th grade. The most recent pre-lottery score in a subject defines a student's baseline score. For the observational analysis, outcome grades are 5th through 8th for middle school 10th for high school; baseline scores are from 4th grade for middle school and 7th or 8th grade for high school.

B Matching Data Sets

B.1 Match from the MCAS to the SIMS

The processed SIMS and MCAS files were merged by grade, year, and a state student identifier known as the SASID. Scores that could not be matched to the SIMS were dropped. This restricted eliminated 0.7 percent of MCAS scores statewide.

B.2 Match from the Lottery Records to the State Database

Match procedure

Lottery records were matched to the state SIMS/MCAS database by name, application year, and application grade. In some cases, this procedure did not produce a unique match. We accepted some matches based on fewer criteria where the information on grade, year, and town of residence seemed to make sense.

Match success rate

Our matching procedure successfully located most applicants in the SIMS database. Table A7 reports cohort-specific match rates from the lottery records to the combined SIMS/MCAS file, separately for middle and high school. The overall match rates for middle and high school were 92.1 percent and 93.7 percent, respectively. Table A7 also reports separate match rates for offered and non-offered students. In middle school, offered students were slightly more likely to be matched (94.0 percent compared to 89.4 percent). Offered and non-offered applicants to charter high schools were matched to the SIMS at almost similar rates (94.0 percent compared to 93.3 percent).

C Construction of the Outcome Data Sets

C.1 Lottery Sample

Further sample restrictions

Once matched to the SIMS, each student is associated with a unique SASID; at this point, we can therefore determine which students applied to multiple schools in our lottery sample.

Following the match, we reshape the lottery data set to contain a single record for each student. If students applied in more than one year to lotteries at a particular school level (middle or high), we keep only the records associated with their first year of application. In our basic lottery analyses, we also exclude students without baseline demographics in the SIMS; in effect, this rule limits the sample to students in Massachusetts' public schools at baseline. Rows 6-9 in each panel of Table A6 report the impact of these restrictions on sample sizes for middle and high school. The set of matched first-time applicants with baseline demographics includes 7,530 middle school students and 5,260 high school students.

Final set of outcomes and students

To generate the middle school analysis file, the matched lottery/SIMS/MCAS file is reshaped to long format, with each observation referring to a test score outcome for a student in a particular year. The high school analysis file uses only 10th grade outcomes, so it includes a single observation for each student. Table A8 summarizes the analysis files for middle and high school. Columns (1) and (2) list the application and outcome grades for each cohort, and column (3) lists the number of applicants satisfying the sample restrictions from Table A5. In middle school, 7,307 of 7,530 students contribute at least one test score to the analysis. In high school, 4,025 of 5,260 students have at least one score. Middle school applicants contribute different numbers of scores to the analysis depending on their years and grades of application; math and ELA tests were not given in every middle school grade until 2006, and some cohorts are not observed through 8th grade. Table A9 lists the grades and years in which math and ELA subjects were administered. As shown in columns (5) through (8) of Table A8, we find 16,543 out of 18,798 expected scores for middle school math, 16,285 of 18,515 for middle school ELA, 4,047 of 5,260 for high school math, and 4,100 of 5,260 for high school ELA. These outcomes are used to produce the 2SLS estimates reported in Tables 5 through 8.

C.2 Observational Sample

To produce the analysis file used for the observational analysis, we begin with the matched SIMS/MCAS state database. As described in Section V, we define cells based on baseline school, baseline year, race, and sex, separately for middle school and high school. We then count the number of students in each cell who go on to spend time in eligible charter schools and regular public schools in the relevant range of grades (5th through 8th for middle school and 10th for high school). Observations in cells that do not include at least one student who attends eligible charter schools and one student who attends regular public schools are dropped. We then produce a long format data file containing the full set of test score outcomes for the remaining sample of matched students at the relevant school level, as well as variables counting years of attendance at each eligible charter school. This file is used to produce the observational estimates. Our matching procedure excludes 23 percent of students who attend eligible charter schools in middle or high school.

D Sample for NCHS

Our case study of NCHS uses lottery data from kingergarten entrance lotteries held between 2004 and 2007, as well as 5th, 7th, and 10th grade lotteries held in 2005 and 2010. To construct the analytic sample, we follow the same data assembly and matching procedures described above. The processing and construction of lottery data for NCHS is detailed in tables A12 through A14. The lottery records include 977 records that meet our sample restrictions. Table A13 shows that we find matches for 92.8 percent of these applicants in the SIMS (95.6 percent for offered students, 90.8 percent for non-offered students). After eliminating unmatched students and repeat applicants, we are left with 902 randomized applicants (see the last row of Table A12). As reported in Table A14, our analytic sample includes scores for 778 of these students. We find 1501 out of 1641 expected scores in both math and ELA.

Table A1: Massachusetts Charter Schools Eligible for the Lottery Study

School (1)	Town (2)	Urban (3)	Grades (4)	Eligible middle (5)	Eligible high (6)	Years in lottery study (7)
Academy of the Pacific Rim Charter School	Boston	Yes	5-12	Yes		2005-2010
Advanced Math and Science Academy Charter School	Marlborough		6-12	Yes		
Barnstable Horace Mann Charter School	Marstons Mills		4-5	Yes		
Berkshire Arts and Technology Charter Public School	Adams		6-12	Yes		
Boston Collegiate Charter School	Boston	Yes	5-12	Yes	Yes	2002-2010
Boston Preparatory Charter Public School	Boston	Yes	6-11	Yes		2005-2010
Cape Cod Lighthouse Charter School	Orleans		6-8	Yes		2007-2010
Christa McAuliffe Regional Charter Public School	Framingham	Yes	6-8	Yes		
City on a Hill Charter Public School	Boston	Yes	9-12		Yes	2002, 2004-2009
Codman Academy Charter Public School	Boston	Yes	9-12		Yes	2004, 2008-2009
Community Charter School of Cambridge	Cambridge	Yes	7-12	Yes		
Dorchester Collegiate Academy Charter School	Boston	Yes	4-5	Yes		
Edward Brooke Charter School	Boston	Yes	K-8	Yes		2006-2009
Excel Academy Charter School	Boston	Yes	5-8	Yes		2008-2010
Four Rivers Charter Public School	Greenfield		7-12	Yes	Yes	2003-2010
Francis W Parker Charter Essential School	Devins		7-12	Yes		2006-2010
Global Learning Charter Public School	New Bedford	Yes	5-12	Yes		2006-2007, 2009
Hampden Charter School of Science	Chicopee	Yes	6-10	Yes		
Health Careers Academy Charter School	Boston	Yes	9-12		Yes	
Innovation Academy Charter School	Tyngsboro		5-11	Yes		2007-2010
KIPP Academy Lynn	Lynn	Yes	5-8	Yes		2005-2009
Marblehead Community Charter Public School	Marblehead		4-8	Yes		2005-2007, 2010
MATCH Charter Public School	Boston	Yes	6-12	Yes	Yes	2002-2010
New Leadership Charter School	Springfield	Yes	6-12	Yes		
North Central Charter Essential School	Fitchburg	Yes	7-12	Yes		
Phoenix Charter Academy	Chelsea	Yes	9-12		Yes	
Pioneer Charter School of Science	Everett	Yes	7-11	Yes		
Pioneer Valley Performing Arts Charter Public School	South Hadley		7-12	Yes		2006-2010
Rising Tide Charter Public School	Plymouth		5-8	Yes		2009
Roxbury Preparatory Charter School	Boston	Yes	6-8	Yes		2002-2010
Salem Academy Charter School	Salem		6-12	Yes		2010
Smith Leadership Academy	Boston	Yes	6-8	Yes		
Sturgis Charter Public School	Hyannis		9-12		Yes	2004, 2006, 2008-2009

Notes: This table lists all charter schools in Massachusetts eligible for the lottery study. To be counted as eligible, a school must be open in the relevant years and meet the entry grade and student population restrictions required for inclusion in column (3) of Table 1.

Table A2: Covariate Balance

	Middle school		High school	
	All lotteries	Lotteries with baseline scores	All lotteries	Lotteries with baseline scores
	(1)	(2)	(3)	(4)
Hispanic	0.020**	0.023**	0.007	0.000
	(0.010)	(0.010)	(0.015)	(0.015)
Black	-0.005	-0.011	0.001	0.010
	(0.011)	(0.011)	(0.016)	(0.017)
White	-0.006	-0.005	-0.002	-0.004
	(0.009)	(0.010)	(0.010)	(0.010)
Asian	0.003	0.003	-0.001	-0.002
	(0.004)	(0.004)	(0.006)	(0.006)
Female	0.009	0.010	0.007	0.010
	(0.014)	(0.014)	(0.018)	(0.018)
Subsidized Lunch	0.005	0.007	0.029*	0.023
	(0.011)	(0.012)	(0.015)	(0.016)
Special Education	-0.003	-0.004	0.004	-0.002
	(0.011)	(0.011)	(0.014)	(0.014)
Limited English Proficiency	0.000	0.001	0.009	0.009
	(0.007)	(0.008)	(0.006)	(0.006)
Baseline ELA Score	-	-0.006	-	-0.041
		(0.027)		(0.032)
Baseline Math Score	-	-0.015	-	-0.012
		(0.026)		(0.034)
p-value, from F-test	0.367	0.452	0.595	0.741
N	7530	7060	5261	4671

Notes: This table reports coefficients from regressions of the variable in each row on an indicator variable equal to one if the student won the lottery. Regressions include risk set dummies and baseline grade dummies and exclude students with sibling priority and late applicants. Samples in columns (1) and (3) are restricted to students from cohorts where we should observe at least one test score. Samples in columns (2) and (4) are restricted to students who also have baseline test scores. F-tests are for the null hypothesis that the coefficients on winning the lottery in all regressions are all equal to zero. These test statistics are calculated for the subsample that has non-missing values for all variables tested.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table A3: Attrition

School level	Subject		Proportion of non-offered with MCAS (1)	Differential (2)
Middle	ELA		0.903	0.024***
				(0.007)
		N	2933	7530
	Math		0.907	0.019***
				(0.007)
		N	2933	7530
High	ELA		0.766	0.004
				(0.015)
		N	1829	5261
	Math		0.753	0.009
				(0.015)
		N	1829	5261

Notes: This table reports coefficients from regressions of an indicator variable equal to one if a student has a followup test score on an indicator variable equal to one if the student won the lottery. Column (1) shows the fraction of non-offered students with followup scores, while column (2) shows the differential by offer status. Regressions include risk set dummies as well as demographic variables, year of birth dummies, year of baseline dummies, and baseline grade dummies. The sample is restricted to students who participated in an effective lottery from cohorts where we should observe follow-up scores.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table A4: Comparison of Lottery and Observational Estimates for Eligible Charters

			Urban			Non-urban		
				Observational estimates			Observational estimates	
School level	Subject		Lottery estimate (1)	Lottery sample (2)	Non-lottery sample (3)	Lottery estimate (4)	Lottery sample (5)	Non-lottery sample (6)
Middle	ELA		0.146***	0.158***	-0.035***	-0.144***	-0.009	-0.013
			(0.028)	(0.010)	(0.012)	(0.039)	(0.007)	(0.011)
		N	11649	131136		4636	239288	
	Math		0.321***	0.249***	-0.024*	-0.123***	-0.015**	-0.008
			(0.031)	(0.013)	(0.014)	(0.047)	(0.007)	(0.012)
		N	11941	136046		4602	248711	
High	ELA		0.280***	0.261***	0.103***	-0.048	0.068***	-
			(0.071)	(0.020)	(0.018)	(0.059)	(0.015)	
		N	3565	7940		536	14357	
	Math		0.360***	0.304***	-0.011	-0.023	0.043**	-
			(0.083)	(0.036)	(0.018)	(0.071)	(0.018)	
		N	3517	7753		531	14274	

Notes: This table reports estimates of the effects of years in charter schools on test scores. Eligible charters are schools with entry grades 4-7 (middle) or 9 (high), and that meet the other restrictions from Table 1. The sample is produced by matching charter students to students in traditional public schools on cells defined by sending school, baseline year, and baseline demographics (race, sex, limited English proficiency, special education status, and free lunch status). All models control for cell fixed effects, year effects, grade effects, and baseline test scores. Middle school regressions pool outcomes from 5th through 8th grade and cluster by student identifier as well

*significant at 10%; **significant at 5%; ***significant at 1%

Table A5: Lottery Models for Charter School Treatment Effects

	ELA				Math			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	0.035	-0.123	0.027	-0.060	0.071**	-0.295*	0.059**	-0.072
	(0.024)	(0.085)	(0.023)	(0.119)	(0.029)	(0.159)	(0.028)	(0.220)
Urban	0.082***	0.049	-0.009	0.044	0.191***	0.067	0.060	0.028
	(0.022)	(0.032)	(0.029)	(0.062)	(0.029)	(0.044)	(0.038)	(0.091)
High school	0.061	0.016	0.044	-0.029	-0.019	-0.061	-0.044	-0.048
	(0.046)	(0.049)	(0.045)	(0.054)	(0.054)	(0.060)	(0.053)	(0.064)
Minutes per day/60	-	-0.021*	-	-0.024*	-	-0.030	-	-0.054*
		(0.013)		(0.013)		(0.024)		(0.029)
Minutes in subject/60	-	0.006	-	0.001	-	0.130	-	0.267**
		(0.027)		(0.028)		(0.092)		(0.104)
PPE/1000	-	0.026***	-	0.017*	-	0.039***	-	0.011
		(0.009)		(0.010)		(0.013)		(0.015)
No Excuses	-		0.127***	0.075**	-		0.183***	0.113***
			(0.028)	(0.032)			(0.035)	(0.032)
Peer baseline score in subject	-	-	-	-0.155	-	-	-	-0.158
				(0.148)				(0.157)
Peer fraction free lunch	-	-	-	-0.099	-	-	-	-0.137
				(0.211)				(0.209)
Black/hispanic	-	-	-	0.057	-	-	-	0.071
				(0.043)				(0.043)
Black/hispanic*peer baseline score	-	-	-	0.233	-	-	-	-0.009
				(0.170)				(0.177)
Black/hispanic*peer fraction free lunch	-	-	-	0.568**	-	-	-	0.290
				(0.288)				(0.267)
N (scores)	17845	17221	17845	17221	17955	17324	17955	17324
N (students)	9305	9005	9305	9005	9273	8974	9273	8974

Notes: This table reports 2SLS estimates of the effects of years of attendance at eligible charter schools and its interactions with school, student, and peer characteristics. The instruments are interactions of each characteristic with the offer dummy. For school characteristics, the attributes of the schools a student applied to are used to construct the instruments. Peer characteristics are baseline characteristics of students in the risk set, and are computed via jacknife (that is, each student is omitted from the calculation that produces her own peer mean). Peer variables are mean zero in the estimation sample. The middle school and high school observational samples are stacked. All models control for grade effects, year effects, risk set effects, student characteristics. Main effects of school characteristics are not included in the regressions, while main effects for student and peer characteristics are included. Standard errors are double-clustered by student and school-year-grade.

*significant at 10%; **significant at 5%; ***significant at 1%

Table A6: Sample Restrictions for the Lottery Analysis

	Lottery cohort									
	2002-2003	2003-2004	2004-2005	2005-2006	2006-2007	2007-2008	2008-2009	2009-2010	2010-2011	All lotteries
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Middle School										
Total number of entry grade records	313	394	391	990	1578	2124	2132	2877	2239	13038
Excluding disqualified applicants	313	394	391	990	1577	2106	2115	2873	2225	12984
Excluding late applicants	313	391	390	972	1551	2046	2054	2829	2222	12768
Excluding applicants from outside of area	313	387	388	963	1540	2028	2041	2741	2202	12603
Excluding siblings	295	358	343	890	1378	1787	1801	2395	1973	11220
Excluding records not matched to the SIMS	267	311	305	838	1311	1710	1669	2095	1825	10331
Reshaping to one record per student	267	311	304	741	1115	1505	1424	1757	1568	8992
Excluding repeat applications	267	308	302	728	1093	1470	1360	1705	1497	8730
In Massachusetts public schools at baseline	201	228	223	603	924	1291	1195	1578	1287	7530
Excluding students without a test score	187	208	210	569	883	1219	1129	1475	1080	6960
Panel B. High School										
Total number of entry grade records	775	717	1313	1219	1148	1411	1392	1531	-	9506
Excluding disqualified applicants	775	717	1309	1218	1146	1408	1391	1520	-	9484
Excluding late applicants	765	710	1280	1215	1138	1408	1372	1517	-	9405
Excluding applicants from outside of area	765	706	1278	1206	1134	1403	1372	1504	-	9368
Excluding siblings	732	677	1218	1165	1120	1362	1334	1401	-	9009
Excluding students not matched to the SIMS	645	614	1121	1074	1091	1306	1255	1321	-	8427
Reshaping to one record per student	573	614	895	852	834	936	863	937	-	6504
Excluding repeat applications	573	612	891	846	812	919	830	895	-	6378
In Massachusetts public schools at baseline	406	462	732	690	692	821	715	742	-	5260
Excluding students without a test score	328	358	583	519	567	659	574	537	-	4125

Notes: This table summarizes the sample restrictions imposed for the lottery analysis. Disqualified applications are defined as duplicate records and applications to the wrong grade.

Table A7: Match from Lottery Records to SIMS

	Number of records	Fraction with SIMS match		
Lottery cohort		Total	Offered	Not offered
	(1)	(2)	(3)	(4)
		Panel A. Middle School		
2002-2003	295	0.908	0.934	0.859
2003-2004	358	0.869	0.882	0.817
2004-2005	343	0.889	0.924	0.849
2005-2006	890	0.942	0.967	0.886
2006-2007	1378	0.951	0.962	0.933
2007-2008	1787	0.957	0.978	0.917
2008-2009	1801	0.927	0.958	0.881
2009-2010	2395	0.875	0.865	0.884
2010-2011	1973	0.925	0.950	0.901
All	11220	0.950	0.940	0.894
		Panel B. High School		
2002-2003	732	0.898	0.911	0.831
2003-2004	677	0.907	0.879	0.932
2004-2005	1218	0.922	0.934	0.893
2005-2006	1165	0.922	0.937	0.901
2006-2007	1120	0.974	0.977	0.971
2007-2008	1362	0.959	0.965	0.955
2008-2009	1334	0.941	0.939	0.951
2009-2010	1401	0.939	0.956	0.932
All	9009	0.937	0.940	0.933

Notes: This table summarizes the match from the lottery records to the SIMS data. The sample excludes disqualified applicants, late applicants, out-of-area applicants, and siblings.

Table A8: Outcome Data for the Lottery Analysis

Lottery cohort	Application grades (1)	Outcome grades (2)	Number of applicants (3)	Number with a test score (4)	Number of math scores expected (5)	Number of ELA scores expected (6)	Number of math scores observed (7)	Number of ELA scores observed (8)
Panel A. Middle School								
2002-2003	5-6	6-8	201	187	402	290	351	253
2003-2004	5-7	6-8	228	208	510	418	433	356
2004-2005	5-7	6-8	223	210	619	547	542	472
2005-2006	4-7	4-8	603	569	2115	2115	1894	1894
2006-2007	4-7	4-8	924	883	3037	3037	2693	2700
2007-2008	4-7	4-8	1291	1219	4287	4280	3724	3701
2008-2009	5-7	5-8	1195	1129	3385	3385	2950	2956
2009-2010	5-7	5-8	1578	1475	3156	3156	2856	2850
2010-2011	4-7	4-8	1287	1427	1287	1287	1100	1103
All	4-7	4-8	7530	7307	18798	18515	16543	16285
Panel B. High School								
2002-2003	5,9	10	406	328	406	406	327	328
2003-2004	5,7,9	10	462	258	462	462	352	356
2004-2005	7,9	10	732	583	732	732	569	579
2005-2006	7,9	10	690	519	690	690	507	514
2006-2007	9	10	692	567	692	692	561	562
2007-2008	9	10	821	659	821	821	637	657
2008-2009	9	10	715	574	715	715	564	570
2009-2010	9	10	742	537	742	742	530	534
All	5,7,9	10	5260	4025	5260	5260	4047	4100

Notes: This table summarizes observed test score outcomes for charter school lottery applicants. The sample is restricted to randomized applicants matched to baseline SIMS demographics. Expected test scores are post-lottery scores in grades 4-8 for middle school and grade 10 for high school that would be taken in Spring 2010 or earlier given normal academic progress after the lottery. Table A1 lists the schools participating in each cohort and their entry grades. Table A7 lists the availability of math and ELA tests by year.

Table A9: Availability of MCAS ELA and Math Tests by Year

Subject	School year	4th grade (1)	5th grade (2)	6th grade (3)	7th grade (4)	8th grade (5)	10th grade (6)
ELA	2001-2002	Yes			Yes		Yes
	2002-2003	Yes			Yes		Yes
	2003-2004	Yes			Yes		Yes
	2004-2005	Yes			Yes		Yes
	2005-2006 through 2010-2011	Yes	Yes	Yes	Yes	Yes	Yes
Math	2001-2002	Yes		Yes		Yes	Yes
	2002-2003	Yes		Yes		Yes	Yes
	2003-2004	Yes		Yes		Yes	Yes
	2004-2005	Yes		Yes		Yes	Yes
	2005-2006 through 2010-2011	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the years and grades in which MCAS ELA and math tests were administered between 2002 and 2011.

Table A10: Covariate Balance for NCHS

	Offer coefficient
	(1)
White	-0.050
	(0.033)
Asian	0.019
	(0.021)
Black	0.017
	(0.024)
Female	-0.063
	(0.039)
Free/reduced price lunch	0.041
	(0.026)
Special Education	0.004
	(0.024)
Limited English Proficiency	0.005
	(0.010)
p-value from F-test	0.418
N	712

Notes: This table reports coefficients from regressions of demographic characteristic on a dummy for receiving an offer from NCHS, controlling for risk sets (dummies for year of application*grade of application*contemporaneous sibling applicant). F-statistics and p-values are from tests of the hypothesis that all coefficients are equal to zero. Demographics for middle and high school are measured in the grade prior to the lottery; demographics for elementary school are measured in kindergarten.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table A11: Attrition for NCHS

Subject	Proportion of non-offered with MCAS (1)	Differential (2)
ELA	0.875	-0.044**
		(0.020)
		712
Math	0.874	-0.040**
		(0.020)
		712

Notes: Column (1) reports the fraction of non-offered NCHS applicants with at least one test score. Column (2) reports coefficients from regressions of an indicator variable equal to one if the student has a test score on an indicator variable equal to one if the student won the lottery. Regressions include risk set dummies, race dummies, and sex dummies. Robust standard errors are reported in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table A12: NCHS Lottery Records

	Lottery cohort					
	2004	2005	2006	2007	2010	All lotteries
	(1)	(2)	(3)	(4)	(5)	(6)
Total number of records	196	312	182	256	263	1209
Excluding siblings	152	263	133	187	242	977
Excluding students not matched to the SIMS	146	247	127	176	211	907
Excluding repeat applicants	146	247	125	175	209	902

Notes: This table summarizes the raw NCHS lottery data. The top row gives the total number of records, and each successive row adds sample restrictions. The second row eliminates students with sibling priority. The third row eliminates students who cannot be matched to the SIMS database. The fourth row excludes the second record for student who applied to NCHS more than once.

Table A13: Match from NCHS Lottery Data to SIMS

Lottery cohort	Number of students (1)	Fraction with SIMS match: Total (2)	Offered (3)	Not offered (4)
2004	152	0.961	0.966	0.957
2005	263	0.939	0.967	0.915
2006	133	0.955	0.988	0.900
2007	187	0.941	0.974	0.932
2010	242	0.872	0.908	0.842
All cohorts	977	0.928	0.956	0.908

Notes: This table summarizes the match from the NCHS lottery records to the SIMS database. The sample excludes siblings. The offer variable referred to in columns (3) and (4) is ever offer.

Table A14: Outcome data for NCHS Applicants

Lottery cohort	Number of students (1)	Number with an observed test score (2)	Number of test scores expected (3)	Math test scores observed (4)	ELA test scores observed (5)
2004	146	132	584	500	499
2005	247	106	125	106	106
2006	125	118	250	234	233
2007	175	155	175	162	162
2010	209	118	141	117	118
All cohorts	902	778	1641	1501	1501

Notes: This table summarizes observed test score outcomes for NCHS applicants. The sample is restricted to first time non-sibling applicants who are matched to the SIMS. Column (2) reports the number of students for whom at least one outcome is observed. Column (3) gives the number of test scores that should be observed (for both Math and ELA) given each applicant's lottery cohort and application grade. Columns (4) and (5) report the numbers of post-lottery math and ELA outcomes that are observed in the data.

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